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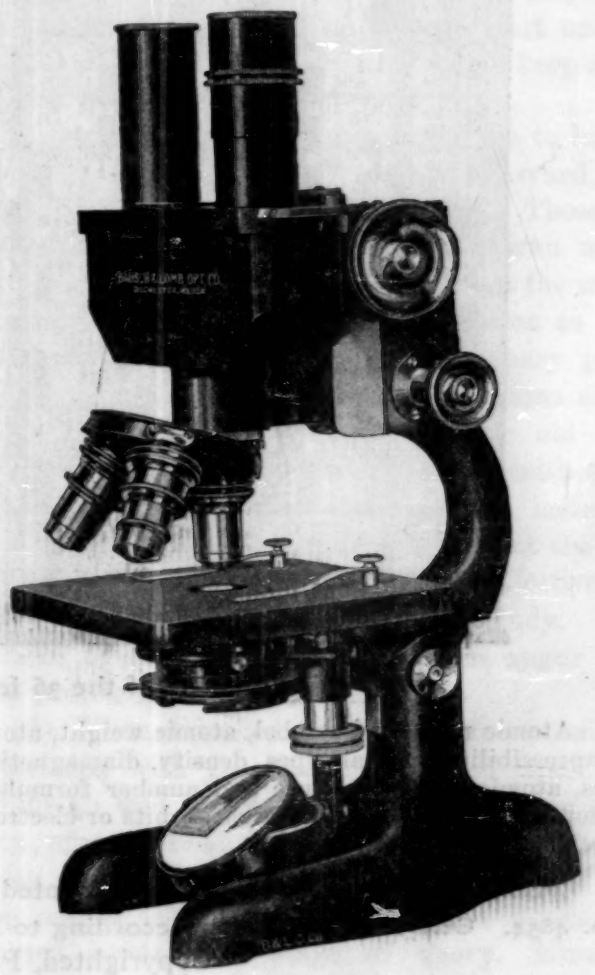

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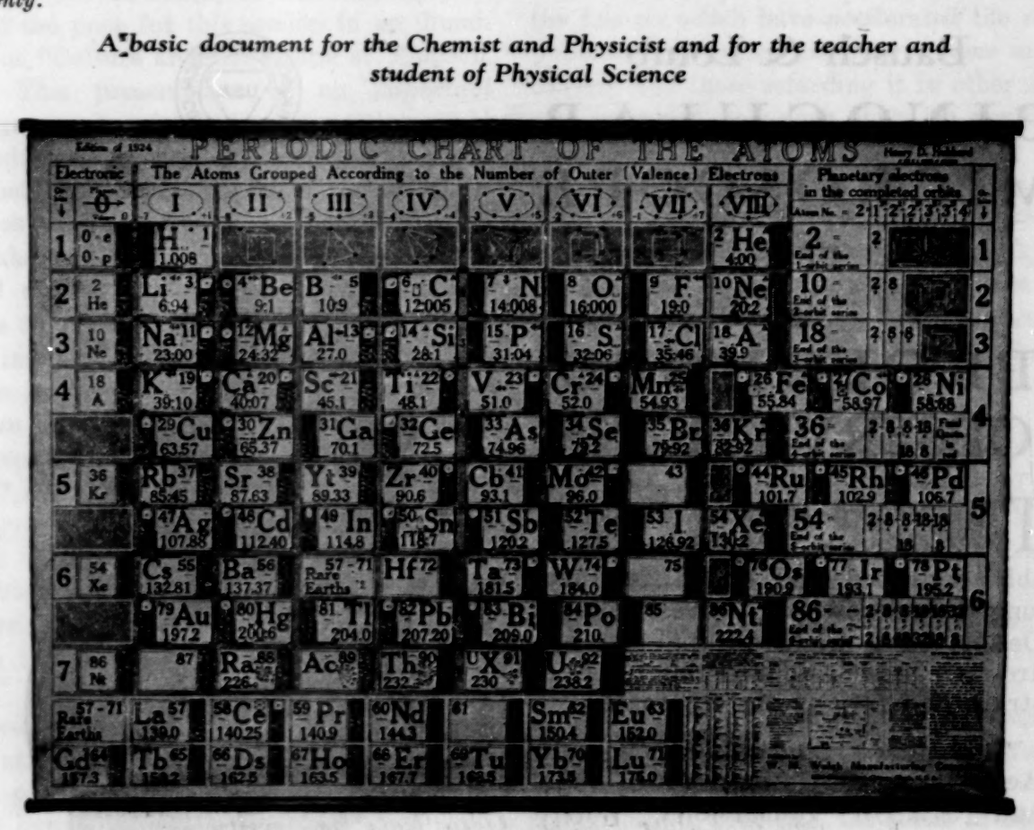

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VOL. LXIII MARCH 19, 1926 No. 1629

CONTENTS

Statistical Inference: PROFESSOR EDWIN B. WILSON 289
Biological Research at the Scripps Institution of Oceanography: DR. T. WAYLAND VAUGHAN 297
Roger Webb Gannett: PROFESSOR S. G. BERGQUIST ... 297
Scientific Events:
Russian Scientific Activities; J. B. S. Haldane and the University of Cambridge; The Ella Sachs Plotz Foundation; Research in Cerebral Physiology at the University of Iowa; The American School of Prehistoric Research 298
Scientific Notes and News 300
University and Educational Notes 305
Discussion and Correspondence:
Black Chaff of Wheat in Russia: DR. ERWIN F. SMITH. *A Seismological Note:* PERRY BYERLY. *The Treatment of Wart Disease of Potatoes with Sulphur:* W. A. ROACH and WM. B. BRIERLEY. *The Attitude of the Electrical Railway Companies on Electrolysis:* IRA D. VAN GIESEN 305
Scientific Books:
Poggendorff's History of the Exact Sciences: FELIX NEUMANN. *Report of the Committee for the Investigation of Atmospheric Pollution:* PROFESSOR ALEXANDER MCADIE 309
Special Articles:
The Ions of Inert Gases as Catalysts: DR. S. C. LIND and DR. D. C. BARDWELL. *The New Status of Net Energy Determination:* PROFESSOR E. B. FORBES. *Some Ecological Relations of the Hypogaeous Fungi:* PROFESSOR W. A. SETCHELL and MARTHA GERTRUDE WATSON 310
Societies and Academies:
The American Society of Naturalists: PROFESSOR A. FRANKLIN SHULL. *The American Mathematical Society:* PROFESSOR R. G. D. RICHARDSON. *The Western Meeting of the American Mathematical Society:* PROFESSOR ARNOLD DRESDEN 315
Science News x

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STATISTICAL INFERENCE[1]

(1) IT was two years ago almost to a day that I was last here at the Johns Hopkins University for a brief visit. It was a pleasure to come then and now to be here once more, only I miss that kind soul, Dr. Halsted, and so must you, very much.

We are both engaged in a new undertaking—the teaching in a large way of the sciences basal to the public health. In a preliminary, a small, but I think very useful way we at Harvard, in cooperation with the Massachusetts Institute of Technology, started systematic instruction, I think, before you did, but to you undoubtedly will be given the credit, and quite properly, of making the first large start under your perennial leader, Dr. Welch. We shall keep step with you as well as we can.

And may I say that it appears to me to be of very good omen that it is Hopkins and Harvard who are working together along this new line. These two institutions have been leaders in American university education, leaders in their insistence on the university point of view, on scientific investigation as the normal university life and as the necessary precursor and accompaniment of sound applications and effective teaching of knowledge, whether old or new. There is in the pursuit of the public health unlimited opportunity for the satisfaction of a natural emotional desire to aid our fellow man, but the aid will be most effective, most permanently effective only if it is forwarded on patient scientific study. The traditions of our respective institutions augur well for the proper grounding of our work.

(2) When I looked about to choose a subject for discussion with you to-day it seemed to me that we might do well to consider a while together the question of statistical inference. Advisedly I say consider the question of inference, for I doubt whether the matter is yet sufficiently settled so that it has passed beyond the stage of query. Moreover, it seemed as though I should be likely to find here an audience not unwonted to reflect on this problem. Forty years ago you had Charles S. Peirce, a leader in the study of logic and one who did not shun the difficulties of probability and of statistical inference. You also drew on Simon Newcomb, who pondered much on the intricacies of the reduction of observations and was so astute in his own reductions that despite the advances of physics and astronomy some of his determinations have not been bettered to this

[1] DeLamar Lecture, School of Hygiene and Public Health, Johns Hopkins University, February 25, 1924.

day. And you have now the most active and scientific department of biometry and vital statistics in America and perhaps in the world, for I am not sure that we have to make any exception of the laboratory of Karl Pearson, the founder of this science.

Indeed, although I am thus assured of an especially informed audience, and because I am, I feel somewhat as Walter Nernst did on beginning his lectures on thermodynamics at Yale some fifteen years ago when after a reference to the work of Willard Gibbs he remarked that he was very keen to come there to expound this subject, meaning by keen, I suppose, *kühn*.

(3) Let us start with ordinary syllogistic deduction and with a simple example. The syllogism consists of three statements, two of which are premises and the third conclusion.

First premise:	All men are mortal.
Second premise:	Socrates is a man.
Conclusion:	Socrates is mortal.

The conclusion follows ineluctably from the premises. It follows whether the premises be right or wrong. Even if all men were not mortal and Socrates were not a man, the conclusion would follow. Thus

First premise:	All men are oysters.
Second premise:	Socrates is a man.
Conclusion:	Socrates is an oyster.

Or again:

First premise:	All men are mortal.
Second premise:	Cerberus is a man.
Conclusion:	Cerberus is mortal.

It is the very essence of syllogistic deduction that it is sure. One may say that we have here a form or formula of deduction. The method of thought can not lead one into error. Any sources of error must be found elsewhere than in the deduction; they must lie in the premises.

Now it should be clear that it is advantageous to separate our liabilities to error. Perhaps we can never be entirely certain of both our premises and so we may not be sure of the truth of our conclusion. Yet we are sure of its truth if the premises are true. We may introduce explicitly into the syllogistic formula our possible doubts as to the premises by making them hypothetical: *If* all men are mortal, and *if* Socrates is a man, then Socrates must be mortal.

(4) Pure or abstract mathematics shares with logic this distinction of being unerring in its deductions.[2] I do not mean that a mathematician may not blunder —we are all frail creatures. But the mathematician is concerned with forms of drawing conclusions and with the precision of those forms, not with the existential content of the premises or the conclusions. There was a long discussion in geometry as to the truth of Euclid's parallel postulate. The termination came when the non-Euclidean geometries were advanced about a century ago, geometries in which the parallel postulate was replaced by a variant and which were none the less logically consistent within themselves. There is no mathematical sense to the question: Is the parallel postulate true? Whatever sense there is is physical, *i.e.*: Do we have the most satisfactory geometry for the codification of natural laws when we use the Euclidean system?

The advantage of mathematics to the person who would apply it is precisely in the certainty of its analysis; it does not guarantee conclusions but serves to separate the processes of drawing conclusions from the difficulties inherent in the acceptance or rejection for practical purposes of the premises. The whole business of pure mathematics is to be self-consistent; truth and falseness do not enter into its field. Herein, too, lies the disadvantage of mathematics in the sciences. For in order to apply any formula or conclusion of mathematics it is necessary first to convince oneself that the formula or conclusion is in truth applicable to the case in hand. Logically we should have to verify the truth for the case in hand of every premise in the whole argument by which the formula had been derived—we could trust the professional reputation of the mathematician as to the consistency of his deduction, but we have ourselves the responsibility of verifying the various premises.

(5) Practically such verification is impossible for two reasons. First, the chain of argument is often so long and often so unintelligible as to wear us out; but there is another and more serious difficulty. That which I have stated as the manner of mathematics is really but an ideal manner, a party etiquette, not an everyday habit. Few mathematicians actually state all their premises in any field of work, and in many fields of mathematical prowess those premises are not even precisely known by anybody. They are to-day known for arithmetic of several kinds, algebra of several kinds, geometry of several kinds. I doubt if they are known for mechanics or any branch of physics. How forsooth is one who would apply a formula to verify premises which no one has yet known how to state?

What we actually do is to use the formula and trust to its being right for our case; but the responsibility is ours. It is we who assert that for our case the formula should give the right result. Unfortunately there are many persons who do not seem to realize their responsibility in this matter. They seem for some reason to believe that a mathematical formula is eternally true. Their attitude is Shamanistic. They go through with magic propitiatory rites,

[2] See, for example, B. Russell, "The Principles of Mathematics," 1903, Chap. I.

idolatrous of mathematics, ignorant of what it can and can not do for them. And I am not quite sure that the high priests of this pure and undefiled science do not somewhat aid and abet the idolatry.

(6) Many of us do not stop to think what very simple mathematical formulas may break down in practice. Consider, for example, $100 = 2 \times 50$. Is this true? It is of course neither true nor false in general. How about a particular case? If I have fifty apples in each of two baskets, I have one hundred apples altogether. The result is true. The enumeration of apples is something to which the theory of cardinal number is applicable. Consider another case. On one day I notice that the temperature is 100° F. in the shade, and on another day I see that it is 50°. Is the temperature twice as much the first day as it is the second day. It is not. The formula is false this time. Or again I mark one student 100 and another 50. Does the first know precisely twice as much as the second? The measurement of temperature and the estimation of knowledge do not lend themselves to the laws of cardinal number. It is no fault of arithmetic that persons who multiply temperatures or marks may obtain bizarre results.[3]

A long acquaintance both with mathematics and with a reasonably wide variety of applications thereof has made me somewhat skeptical of formulas, and I have perhaps unwisely gone so far as to state in print that I do not believe formulas. It is unwise merely because it is shocking to a prevailing Shamanism. When in Rome one really should do as the Romans do, and if one happens to reside among the Ostiaks or Samoyeds of Siberia it is perhaps still wiser to conform. With my audience to-day I am however quite safe; for no university has stood more resolutely than the Johns Hopkins for straight, independent thinking on the facts as they are. I shall therefore pursue my course a bit farther.

(7) Nearly a year ago I was invited to speak at Yale on the subject of "The Statistical Significance of Experimental Data." My communication was written somewhat in the lighter vein and it was printed in SCIENCE with disastrous consequences.[4] If Dr. Cattell had been the good friend I have liked to think him, he would have returned the manuscript with kind words of caution born of his profound knowledge of psychology and proper to a dignitary about to be elevated to the pontificate of organized science in America. He might have pointed out that SCIENCE was not a funny paper and have respectfully urged upon me as a more appropriate medium of publication either *Life* or *Punch*. To be sure, that might have given me the opportunity to retort that I did not consider *Life's* labored anti-vaccination or anti-vivisection material half so humorous as the anti-Bryan round-robin on evolution recently printed in SCIENCE. Dr. Cattell is too wily to lay himself open to any such thrust, and yet too kindly to suggest, as have some good friends, that my address was fit only for a broad Latin comic *a l'assiette au beurre!*

What were some of the terrible things I said? First and last my main contention was that it took real brains to be a good statistician. This has offended both parties to the context. Let me recant. We are in a mechanistic, nay a mechanical age, and America is the country par excellence of mechanical devices; it is not the illumined and generalizing minds of Farr and Galton that guide us on our way, but the cheerful chatter of the Hollerith tabulator and the Millionair calculator. Then, too, it appears that I slipped in the unfortunate remark that the mature "hunch" of a genius is better than many a scientific demonstration. I must apologize for a low form of hero-worship; it should not be tolerated in a democracy; besides, we are in an industrial era and it has been maintained statistically, has it not, that the best workmen are the morons? Again I recant. But this is aside from my subject to-day.

(8) There was, however, one statement, perhaps the most distressing, which bears on our discussion. I quoted the formula or rule that "the probable error in the mean or average of a number of observations is equal to the probable error of the individual observations divided by the square root of their number." This is the sacred cow of quite a hierarchy of the statistical fraternity. Pray, note that I do not call it the sacred "bull." Experience is a dear teacher, and even I am learning discretion. Later I remarked, just offhand and a bit flippantly, that I did not believe the rule. To be sure, I had shown that it did not work out right in the case in hand. But none the less every brother of the Shamanistic order for the Preservation of the Sanctity of Ancient Icons rose to protect dear bossy from my rough dehorning hands. And just here is where I do not yet recant. I do not believe the formula or rule for the computation of the error in a mean or average from the mean error of the observations. More than that, I do not believe any formula, not even $100 = 2 \times 50$, and particularly when I have already verified for a case in hand that it does not work.

What was my process of showing that the formula was inapplicable? I should have liked to analyze the proof of the rule into all its various steps so that I might examine the many premises to ascertain which ones were violated. This can not be done. Probability and statistics are not yet on a sound logical basis like algebra and geometry. We do not know

[3] We need a finite ordinal arithmetic for our marking system.

[4] SCIENCE, August 10, 1923, Vol. 58, No. 1493, pp. 93–100.

precisely what our premises really are. The best set of postulates or axioms we have is due to Keynes and may be found in Part II of his "Treatise on Probability." We owe him a great deal for this analysis; but this sort of rational founding of a branch of science is very difficult and is the work not of one man; it requires the accumulation of efforts of many students spread over much time.

As is natural in cases where a complete postulational basis has not yet been established and accepted, the proof of the theorem or formula is itself not very clear or convincing, it is an argument rather than a proof. In such cases it is necessary to turn to the end result and see whether in a particular case it seems to work. This is what I did and found that, assuming the formula to be true and applicable, the probability of getting the experimental results that were found was itself very much less than the probability of the conclusion it was desired to establish from the results. Such is the method of *reductio ad absurdum*. We ordinarily accept a statistical thesis as proved if the chances in its favor appear to be better than say twenty to one; but if by using entirely similar calculations on the same data we can show that our data themselves have not one chance in a thousand of existence, we can make no inference other than that the formulas are inapplicable to the material in hand.

(9) There is much of mystery in the situation. We have admirable proofs that no matter what the law of distribution of our observations, the mean or average values of reasonable numbers of the observations are distributed upon the Gaussian or normal curve or error. Edgeworth, who is as clear thinking an investigator as we have in statistics, has worked out simple cases theoretically and has verified them experimentally and has shown how very few need be the number of observations which we average before the mean values do actually distribute themselves very satisfactorily on the standard curve with predetermined parameter.[5] I have worked out such cases myself. There is no doubt, I think, but theoretically and in a great many practical cases the rule that the probable error in the mean is equal to the probable error of the observations divided by the square root of their number is clearly indicated as applicable just as is the rule $100 = 2 \times 50$. Yet I believe there are exceptions, probably broad categories of exceptions, and that the last word has by no means been said on the subject. Probably there are in the theoretical proofs subtle premises, perhaps only tacitly assumed, which if they could be dragged clearly into the light would give us a better understanding of the matter.

In my article in SCIENCE I was dealing with short runs of data such as often occur in experimental work; there were only seven to twelve observations in each series. Inasmuch as many of the proofs of the rule for the error in the mean depend on the assumption that the observations are numerous it might be thought that runs so short as seven to twelve would be adequate reason for not expecting the formula to apply, for excusing us from believing in it in such cases. But Edgeworth's proofs and experiments and some similar investigations of my own seem to indicate a very rapid convergence to regularity in the distribution of the means so that ten observations should be enough to average. There is some mystery lurking here.

(10) Suppose we turn our attention from the short runs of experimental work to the long runs we use in vital statistics. Should we believe the rule for very long runs, for large collections of data? Let us perform an experiment, but only in imagination. Suppose I draw upon the blackboard a line about eight inches long, and let us set about determining the length of that line by the simple process of each one estimating the length. If we could find one hundred compliant individuals and get an independent estimate from each, we could then add all the one hundred values together and divide by one hundred to find the mean estimated length of the line. We may well assume that on the average the length can be estimated to about an inch. Of course some of you would do better than that. But allowing an error in the individual estimate of one inch we should by the formula have for the error in the mean of one hundred estimates only one inch divided by the square root of one hundred, which is ten. The mean should therefore be accurate to the tenth of an inch. We can all believe that this is reasonable.

Imagine, however, that we desire a greater accuracy. We have then merely to go out into the highways and byways and hedges to round up let us say ten thousand interested individuals who could give us estimates. The mean of the ten thousand estimates should be accurate to the hundredth of an inch. And so if we get a million estimates we shall have a mean good to a thousandth of an inch. The process could be continued further to insure a greater accuracy—

[5] Edgeworth, Introductory description (especially pp. vi, vii) to the "Representation of Statistics by Mathematical Formulae," 1900, printed for private circulation and being in the main matter reprinted from *J. Roy. Statist. Soc.*, March, 1900 (among the Miscellanea), Vol. 61, 1898, pp. 670 *ff.*, Vol. 62, p. 125 *ff.*, p. 373 *ff.*, p. 534 *ff.* Reference is made here to Burton, *Phil. Mag.*, Vol. 28, 1889, p. 483, and Edgeworth, *Phil. Mag.*, Vol. 16, 1883, p. 301, *J. Roy. Statist. Soc.*, Vol. 51, 1888, p. 116. One may also consult Bowley, "Elements of Statistics," p. 289, 421.

at least, it could if we still believe the formula. But you may recall that on principle we should not believe formulas; that truth and falseness come in only when the formulas are applied, and that the responsibility lies with those who use the formulas. Is it inherently reasonable that by getting one million fair estimates of the length of the line we should have in their average a value for the length good to one thousandth of an inch? My own faith in the formula does not extend so far.

It would of course be a simple matter to determine by precise physical measurements the length of a line to much greater accuracy than this. Fine mechanical processes require gauging to half a thousandth of an inch or better. But a chalk line on a blackboard is a very indefinite physical object. Did you ever look at one under a reasonable magnification and see that it has neither beginning nor end; that its appearance of continuity and definiteness are due precisely to the fact that we do not examine it under power? How are you by any mathematical formula to determine the length of a line more accurately than the line exists?[6]

(11) Let me leave it as a query. I came not to resolve the difficulties that have their lairs in various corners of this vast labyrinth of probability and statistics, but merely to discuss the matter, to ask questions.

You may think I have hardly a fair experiment when I ask you to estimate the length of the line. Why not measure it, why use such a crude illustration? But note that I was willing to assume that individually you could estimate to an error of around 10 per cent.—one inch in eight. This is not bad accuracy with which to start. I fear my colleague, Dr. Richard Cabot, might not be so generous to the clinician in diagnosis.

Physicists, who are used to precise quantitative measurements under excellent control, are not too sure that the error in a mean can be had by the simple rule. Simon Newcomb, discussing impersonally his determination of the velocity of light, said: "So far as could be determined from the discordance of the separate measures the mean error of Newcomb's result would be less than ± 10 km. But making allowance for the various sources of systematic error the actual probable error was estimated at ± 30 km." He so far lacked confidence in the rule that he allowed a factor of safety of three in stating his precision. Perhaps Newcomb was conservative. From a recent study on which I have collaborated with Dr. W. J. Luyten, of our observatory, it appears that the good-to-best modern photographic stellar parallaxes need in their computed probable errors[7] a factor of safety of only one and one fourth to one and one half.

(12) This sort of knowledge is of course familiar to you. Probably Newcomb himself set it forth here some twoscore years ago. I was taught it in college more than twenty-five years since by Robert W. Willson, and a good chance indeed I had to learn the difference between probable errors computed by rote and rule, from the discordance of the observations, and the presumptive actual probable error. We students had an old meridian circle. It had been a good one in its time but had been neglected and maltreated and then reconditioned. You could find a new source of error in the instrument almost every day to increase your probable error until you thought you were done —and then a hard freeze would come along and heave one of the piers on which it rested. Yet somehow I doubt if with all its age and vagaries this former instrument of precision was less reliable than a modern death certificate. Why is it that we find need in precise physical work for a factor of safety in our probable errors to convert them from hypothetical pure mathematics to presumptive physical facts, while in economic and vital statistics we appear to lay no stress on them? Is it because the worse our data, the sounder our conclusions? It may be.

The question antedates my college days; antedates Newcomb and Peirce. Two centuries and a score of years ago, before Lexis, Quetelet, Gauss or Laplace, it was discussed in general terms between Jacques Bernoulli and Leibnitz. The former was inclined apparently to trust his figures further than the latter. Leibnitz wrote that the estimation of probabilities was very useful but that in affairs of state and many others it was not so much refinement of calculation that counted as an accurate consideration of all the circumstances; that there seemed to him to be an inherent difficulty in the cleancut determination of empirical probabilities because nature, though she had her habits, due to recurrence of causes, did not follow them except in a general way; that new diseases arose and whatever observations you should make on deaths would not thereby constrain nature.

If I should tremble to have the clever eye of Bernoulli fall upon my poor contribution in SCIENCE, I should nevertheless not hesitate with a profound bow of respect to lay a copy on the desk of Leibnitz.

And after all perhaps there is less difference in points of view than the heat of argument, the flush of individual exertion and the glowing dialectic of ill-

[6] It may be of comfort to some to say that we are not determining the length of the line but merely an estimate of its length and that the estimate may well be more precise than the length estimated.

[7] Newcomb, Encyclop. Brit., Vol. 11, p. 625, 11th ed. Wilson and Luyten, Proc. Nat. Acad. Sci., Vol. 10, April, 1924.

defined technical terms might lead us to imagine. Leibnitz and Bernoulli, Pearson and Keynes might practically agree in their practical inferences from the same data. There may be here not so much a difference of science and statistics as of temperament, of endocrine function.

(13) From classical logic we learn that the premises "All men are mortal" and "Socrates is a man" lead ineluctably to the conclusion "Socrates is mortal." We know also that the premises "Some men are liars" and "Munchausen is a man" lead to no conclusion at all. It is here that Charles Peirce[8] comes to his introduction of *probable deduction*. From the premises "Ninety-nine Cretans in a hundred are liars" and "Epimenides is a Cretan" he draws the probable deduction "There are ninety-nine chances in a hundred that Epimenides is a liar."

He does not fail to note that this is a very different sort of conclusion. You do not assert that Epimenides is a liar because he is a Cretan and 99 per cent. of Cretans are liars; but merely that the probability that Epimenides is a liar is ninety-nine in a hundred. Where in deductive logic the conclusion makes a definite attribution of a predicate to the subject, in probable deduction the conclusion makes a statement about probability. It is not true that Epimenides is 99 per cent. a liar. He may be a liar or he may not be; what we assert is that he belongs to a class of individuals ninety-nine out of each one hundred of whom are liars and we abbreviate this to the assertion that the chances he is himself a liar are ninety-nine in a hundred. Peirce emphasizes the fact that we must obtain our premises in good faith. If we happen to know Epimenides personally that may greatly alter our conclusion—even to the affirmation that he is no liar at all.

(14) He next goes on to *statistical deduction*. The premises here are "The proportion p of X's are Y's" and "S is a numerous set taken at random from among the X's," with the conclusion that "probably and approximately the proportion p of the S's are Y's." The inference is not sure but probable, not exact but approximate. The statement of the second premise brings in the term random—a set taken at random from a specified class. He remarks that there is no way of insuring randomness, *i.e.*, fairness or lack of bias in drawing the set of S's from the X's, except by faith in the honesty and open-mindedness of those that make the selection. How would you select one thousand dwellings at random within the city limits of Baltimore? Probably you would not trust your judgment but would resort to a lottery, inscribing on similar slips of paper the street and number of each dwelling, mixing the slips thoroughly and having a blindfolded person select one thousand.

One thing, however, is sure about statistical deduction, namely, that if you persist in repeating your drawings of samples you will ultimately vindicate the conclusion. Indeed the very notion of a random selection is that if the selection is indefinitely repeated you will select each element of the original set the same proportionate number of times. Thus if there are one hundred thousand X's and you select as the set S a particular one thousand not once but very many times at random, you will get any special X in the set S about once in a hundred times. With any specified set which you are sampling this indefinite continuation of the process is conceivable and the result is sure. You verify[9] in the long run that for a random set S the proportion p of elements that are Y is the same as in the class X from which S is selected. And this is precisely what we mean by the statement that in a particular drawing of one set S the proportion is probably approximately equal to p.

(15) When we pass from the larger set to the smaller we deduce and in the long run verify. Suppose we try to pass the other way, ascending from the smaller sample by induction to the larger universe from which it is drawn. The syllogism of *statistical induction* would read as follows: A large random set S is selected from the X's—which is a much larger and in general unknown class. Of the S's the proportion p are Y's. Hence probably and approximately the same proportion p of all the X's are Y's. Clearly it is this process of induction which is used so much in practice. One looks over a considerable number of apples in a barrel, finds a small proportion of poor ones, and buys the barrel with the confidence that the proportion of poor apples is similarly small in the whole lot. Of course in the good old times when the New England farmer "deaconed" his apples you shouldn't sample merely the top layers—and you didn't. Again the fairness of the sample is taken for granted.

In the case of deduction you know the proportion in the whole set and by repeated sampling verify it in the samples. For induction you know the proportion in the sample, and, as Peirce points out, you do not verify it but *modify* it by repeated sampling. This is truly a very great difference. And particularly so inasmuch as the general universe is unknown

[8] C. S. Peirce, "Theory of Probable Inference" in "Studies in Logic by Members of the Johns Hopkins University," Little, Brown and Co., Boston, 1883, pp. 126–203, especially pp. 127, 134, 137, 152, 154, 175. Also the "Probability of Induction," *Pop. Sci. Mon.*, April, 1878, reprinted in "Chance, Love and Logic," Harcourt, Brace and Co., 1923, especially p. 100.

[9] If the verification does not come, within reasonable limits, in due time you have to conclude that your sample is not truly random.

and may be variable. In practice we take a sample S of what we believe to be some large class of X's and find a proportion p of Y's. We take another sample S′ and find the proportion p′ of Y's—and so on to as many samples as we choose to make. When we deal with deduction we can continue the process indefinitely in imagination.

Can we so continue it in the case of induction? Clearly not; for the class of X's which we are sampling is changing like everything else in nature, and we have no means of determining offhand[10] how much of the variation in the successive proportions p, p′, p″ . . . found in the different samples represents fluctuations in the universe itself or fluctuations in or departures from real randomness of our sampling. We can not resort to our lottery, inscribing each element of our unknown universe X on slips of paper, and drawing samples at random. Induction is a much more dangerous process than deduction and one can not be so sure of it. There are limitations, as Leibnitz pointed out to Bernoulli in regard to the perfection, or, as Peirce says, modification of our estimates from sampling as a method of induction.

This may be illustrated by Peirce's own example. He remarks in the returns of the census of 1870 there were among native white infants under one year of age 478,774 males to 463,320 females, a proportion of 0.5082 of males; but among colored children there were 75,985 males to 76,639 females, a proportion of 0.4977 males. He infers that generally there is a larger proportion of male infants among whites than among negroes. That is, we propose to regard the returns of the 1870 census as representing a fair sample as between whites and negroes with respect to the ratio of the sexes of children.

Now it is of the essence of a sample to be a small fraction of the whole universe, and as we have enumerated all the white and colored children in 1870 in these United States we must for our induction consider a universe of all white and all negro infants at all times or in all places or both—or at the very least at many times and in many places. This is the kind of situation that meets us constantly in vital statistics. It is very different from drawing as a sample a cupful of beans out of a bag well shaken up; it is more like picking out as a sample one bag of beans from among a hypothetical class of bags of beans. Still, although recognizing the difference, I do not object to the inference that there are in general relatively more males than females born to whites than to negroes—provided that all the pertinent circumstances have been considered, and I believe that Peirce sufficiently emphasized the necessity of considering them as a matter of common honesty and sound logic.

(16) Later, however, he goes on to what I fear Leibnitz would consider refinement of calculation and states the conclusion that the discrepancy between the proportions 0.5082 for whites and 0.4977 for blacks is so large that the result would happen only once out of ten billion censuses in the long run. Now is there any significance at all to such a statement, and, if so, what is the significance? Clearly it can have nothing to do with vital statistics—with natural phenomena. The best estimate we have of the lapse of time since the earth's crust solidified is about one billion years. I have not seen any estimate of the future duration of the earth as a habitation for either whites or blacks that is longer than that. The human race has probably not existed a million years and may not exist for many millions. Even with the maximum increase of interest in vital statistics which we can stimulate and the maximum multiplication of governmental activities which we can imagine and deplore, ten billion censuses seems a large order. This is, however, but a drop in the bucket, for we are talking of results which would happen only once in ten billion censuses in the long run. Is not the calculation refined to the extreme? Would not a superficial examination of a few of the circumstances surrounding this problem have led us to the conclusion that the calculation could but waste our time and maybe mislead us into thinking that it meant something very different from what it possibly can mean.

Where do we stand logically when we compare Peirce's fundamental logical justification for induction, namely, the self-correcting or modifying property of indefinite repetition of sampling on the one hand, with such a calculation as he makes here on the other hand? Do we not need first to conclude that he is using sampling in two different senses? In modifying an empirically estimated probability by further sampling we are taking new censuses—new in time, new in place, different in numbers of population, etc. But we are taking real censuses and from them we shall get a variety of determinations of the empirical probabilities which we may interpret as best we can. In the second case we are not taking ten billion censuses nor in fact any except the one we have taken; we are doing a piece of pure mathematics which may or may not correspond even with gross approximation to what would happen if repeated censuses were taken.

Indeed the second proposition is not a piece of induction at all but of deduction. The statement is something like this. We imagine two hypothetical universes; one of many millions of white infants of which half are males, another of millions of black infants of which also half are males. By an impar-

[10] There are refined methods of making some such determination, but they are often neglected and of course can not be applied at all when we base our induction on a single sample, as is often done.

tial lottery we draw about one million whites from one universe and about 150,000 blacks from the other, and we call these drawings a census (which is perhaps too graphic an expression). Then we should have but one chance in ten billion of finding so great a difference between the proportions of males and females in the white and negro parts of this artificial census as was actually found in the real census of 1870. We could have as well called the white infants kittens and the black infants mice. We are talking merely about a certain lottery or pair of lotteries, not about blacks or whites or infants or kittens. The theorem is true in the imagined lottery game; it has no truth or falseness relative to actual happenings in actual censuses, and can have none until we have shown, as we can not, that, to the high degree of approximation needed, the actual censuses do justify a calculation based on this lottery.

(17) It is fortunate that there is no physical truth implied by these meticulously accurate calculations. You have heard of the proposition in the theory of probability known as the Gambler's Ruin. Betting ten billion to one would soon ruin one even with the best of luck if he were betting real goods in a real world of actual events; and it would ruin scientific reputations if people didn't realize that we were just playing the high priest in a Shamanistic rite, which is a much safer and perhaps an admirable rôle.

It appears, however, that there are some who can not cheer the game along. Such was the miserable "knocker" of a Leibnitz. Such is that wretched "killjoy" Keynes. He considers it absurd that Pearson after a lengthy analysis should conclude as follows: If a sample of one hundred of a population shows 10 per cent. affected with a certain disease, then in a second sample of one hundred it is even betting that the percentage affected will be between 7.83 and 13.71. Keynes appears to think that Pearson is talking nosometry or vital statistics. Not at all, Mr. Keynes. The proposition is purely mathematical and identically the same as if it had read: If you come across one hundred dairy cows and 10 per cent. of them being milked, then when again you find one hundred dairy cows, it is even money that not fewer than 7.83 of them nor more than 13.71 will be milking. Or again: If you buy one hundred hellgramites and use them up catching ten bass, then you can bet even that in the long run you will come home half your bass fishing days with not less than 7.83 bass nor more than 13.71, provided you use just one hundred hellgramites each day. This form of statement might appeal more than the others to Izaak Walton; but there is no trusting him, because like Mr. Keynes he may be a realist and believe that fishing is fishing and not higher mathematics.

And then there is Whitehead, mathematician, philosopher, physicist and logician par excellence, who in a paper entitled "Uniformity and Contingency" delivered as his presidential address before the Aristotelian Society[11] says: The latest and sublest analysis of the difficulties which cluster around the notion of Induction is to be found in Part III of J. M. Keynes's "Treatise on Probability." What can we say to him? The joint author with Russell of the "Principia Mathematica" can not be suspected for a moment of not appreciating the significance of abstract mathematics. May it be that he believes induction belongs to that real nature which has her habits but only in a general way and that refined calculations mislead? Perhaps he will sometime come and tell us himself.

(18) Fortunately the solution, if there be a solution, to these difficult questions which lie at the basis of statistical theory is not of pressing importance in most practical applications of statistics. There is nothing practical about odds of one in a billion. Had I an adequate grounding in the biological and medical sciences, I might be less concerned with such doubts; but coming as I do to vital statistics through the more exact sciences, I believe that for me to ponder in this way is a very useful help against overstepping or overstumbling the bounds of what is reasonable in drawing conclusions. Did not Pettenkofer demonstrate a relation between the level of the ground water and typhoid in Munich,[12] and were not terrific odds calculated in support of his theory? Was it useful to build the chances quite so towering high that the fall thereof should the more resound?

I seem not to have reached any very definite conclusions; I came but to discuss. And as I remarked at the outset that coming here with any reflections on statistics was but carrying coals to Newcastle, I may only hope at the last that my cargo may not be wholly confiscate as slate.

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[11] *Proc. Aristot. Soc.*, Vol. 23, p. 18.

[12] I do not wish to be misunderstood. There was undoubtedly at Munich at the time a high correlation between the height of the ground water, or variations in the height, with the rise and fall of typhoid; so much is description, not inference. The correlation coefficient many times exceeded its probable error and such an excess in a game of chance would be excessively rare; so much is pure mathematics, not statistical inference. The inference was drawn that there was a direct causative relation, that typhoid entered the body not through the alimentary but through the respiratory tract, that the relation was general both in respect to time and to place and even with respect to disease. This led to a return of *mal-aria* or *miasm* as fundamental in epidemiology, and the point of view prevailed in some quarters for many years and was applied to malaria itself to combat the rising mosquito theory (London, *Epidemiological Society*, Vol. 17, pp. 76–77).

BIOLOGICAL RESEARCH AT THE SCRIPPS INSTITUTION OF OCEANOGRAPHY

As has already been announced in SCIENCE and the newspaper press, the name of the Scripps Institution was changed on October 14, 1925, by the Regents of the University of California from Scripps Institution for Biological Research to Scripps Institution of Oceanography of the University of California. In connection with the change in name it seems desirable to make entirely clear the relations the institution will bear to biological research. Marine biology is a necessary part of any comprehensive program for oceanographic research and in the future as in the past marine biological investigations will constitute an important part of the program of the work of the Scripps Institution. Since an institution with a limited staff can not hope to cover more than a small part of the field of marine biology the investigations of the staff of the institution will be circumscribed. They will be directed chiefly toward the study of marine plankton, the investigation of certain problems in ichthyology, and the study of organisms as geological agents and as a basis of interpreting the record contained in marine sedimentary deposits. It is hoped that the present program of the institution may be considerably expanded so as to gain a better understanding of some of the fundamental factors associated with the complex group of phenomena indicated in the preceding remarks.

Although the actual biological work of the members of the staff of the institution must be limited, such facilities as the institution possesses for research will be most cordially extended to visiting investigators in the future as in the past. During the two years since I have been director, the institution has been used as a base for studies by a number of botanists and zoologists, some of whom were working on marine organisms and others on terrestrial organisms. Among the distinguished zoologists who have worked at the institution since I came here are Professor C. M. Child, of the University of Chicago, and Dr. H. Boschma, of the University of Leiden. Among the botanists were Professor N. L. Gardner, of the University of California, and Miss Kathleen M. Drew, of Manchester, England. I wish not only biologists but other scientific investigators to understand that they will be welcome to utilize the facilities of the Scripps Institution to the limit of its capacity. No attempt has been or will be made to restrict the activities of visiting investigators. They may work on problems in marine biology and terrestrial biology; and I should be glad to have the institution used as a base for oceanographic investigations other than those that are purely biological and for local geological work.

It seems pertinent to make a statement regarding the investigations of Dr. F. B. Sumner. As is well known, Dr. Sumner has for years been engaged on a study of the geographic races and genetics of *Peromyscus,* the deer mice, a genus of rodents. Dr. Sumner will continue his investigations on *Peromyscus* until they have been brought to a logical conclusion. It is obvious that this should be done in order to realize on the large investment of time on the part of Dr. Sumner and the investment of the funds of the institution. Before this investigation has been finished, Dr. Sumner will gradually shift the subject of his researches from *Peromyscus* to fishes. He will undertake genetic studies on aquarium fishes and he will revert to certain problems of the general ecological relations of fishes on which he was engaged before he undertook his researches on *Peromyscus.*

T. WAYLAND VAUGHAN
SCRIPPS INSTITUTION OF OCEANOGRAPHY,
LA JOLLA, CALIFORNIA

ROGER WEBB GANNETT

WITH the passing on November 17, 1925, of Mr. Roger Webb Gannett there was lost to the teaching staff of the Michigan State College an outstanding geologist and instructor.

Mr. Gannett was born in Minneapolis, Minnesota, on June 18, 1896. He was graduated with honors from the University of Minnesota in 1918, receiving the degree of mining engineer. Immediately following his graduation he entered the employ of the Bureau of Mines in Washington, D. C., where he held the position of mineralogist until a few weeks before the signing of the Armistice, when he enlisted in the army. After several weeks spent in camp he was discharged and returned again to the Bureau of Mines, where he continued until May, 1919. At the termination of his services with the Bureau of Mines, Mr. Gannett, with a party of geologists and mining engineers in the employ of the Oriental Mines Company, left for exploration work in Siam. After eight months of mining exploration in Siam, Burma and the Malay Peninsula work was discontinued in this region by the Oriental Mines Company, and Mr. Gannett received orders to join a field party already engaged in geological work in Yunnan, China. In the fall of 1921 he returned from the Orient and spent the following winter in graduate studies at the University of Minnesota. The summer of 1922 as well as the following winter and summer were spent in Alleghany, California, in practical field work as geologist and mining engineer. In January, 1924, he came to the Michigan State College as instructor in geology.

During the fall term of 1925 he was granted a leave of absence to carry on geological work for the government in California. It was there that his death occurred, due to complications following an operation for appendicitis. He was to have resumed his work at the college in January of the present year. His untimely death cut short a life which promised much in the field of science.

S. G. BERGQUIST

SCIENTIFIC EVENTS

RUSSIAN SCIENTIFIC ACTIVITIES

THE following items of scientific interest have been sent to SCIENCE by the Russian Information Bureau, Washington, D. C.

At the first meeting of the Academy of Sciences at Leningrad in 1926 the following honorary members were elected: Professor August Svante Arrhenius, of Stockholm University; Professor Lorentz, of Leyden; Professor Max Planck, of the Prussian Academy of Sciences, and Professor Vito Volterro, president of the International Research Council and of the Italian Academy of Sciences. The following correspondents were also elected: Professor A. A. Ivanov, of the Pulkovo Observatory; Professor A. A. Yakovkin, of the Technological Institute; Professor Arnold Sommerfeld, of Munich; Professor Emil Abderhalden, of Halle; Professor Urbain, of France, and the Professors Glinka and Zelenin, of Leningrad.

The USSR Academy of Sciences has approved the plans submitted by Professor Oldenburg for the organization of several scientific institutions of the USSR in foreign countries. These institutions are intended to serve chiefly as connecting links with the international scientific organizations. Russia had maintained before the revolution several scientific institutions abroad (Constantinople, Rome and Paris), but their work was poorly organized. The new scientific institutions are intended to acquaint foreign scientists with scientific progress in the USSR. It is proposed to establish such institutes at London, Paris, Berlin, Cairo, Washington, Buenos-Aires, Madrid, Peking and Sydney.

The Academy of Sciences is making preparations for the octogenarian jubilee of its president, A. P. Karpinsky. The venerable head of the Academy of Sciences is the first elected president, as in the past the president was an appointee of the Czar. Karpinsky has been a member of the academy for forty years, and its vice-president and president for the last ten years.

The fortieth anniversary was recently celebrated of the scientific career of Professor N. M. Knipovitch, one of the leading Russian authorities in land and sea exploration. Funds have been granted for the publication of a jubilee edition. The celebration took place at Moscow in February.

The forty year jubilee of the scientific career of Professor V. M. Bekhterev, of the USSR Academy of Sciences, was celebrated at Leningrad on December 20. Greetings were delivered by representatives of the central authorities and the federated republics. Telegrams were received from scientists in Germany, France, Italy, Hungary, Yugo-Slavia and America. The occasion is to be commemorated by the establishment of several laboratories and museums dedicated to the name of the eminent scientist.

Professor Shokalsky has returned to Leningrad from a journey to England, Germany, France, Scotland, Belgium, Denmark, Sweden and Finland. At the request of foreign scientific bodies, Professor Shokalsky lectured on geographical researches in the USSR, and on his oceanographical researches, at the French Academy of Sciences, at the Sorbonne, and before the Geographical Societies of Edinburgh and Helsingfors.

Professor Steklov, vice-president of the USSR Academy of Sciences, has returned to Leningrad from a scientific mission to Germany and Italy. Professor Steklov represented the Academy of Sciences at the centenary celebration of the Superior Technical College at Karlsruhe, Germany.

The Russian mineralogist, Professor Vernadsky, of the USSR Academy of Sciences, has returned to Leningrad from Paris, where he studied the properties of radium at the French Academy of Sciences.

Dr. G. Golds, the director of the Bacteriological Institute of Oslo, has arrived in Leningrad on a scientific mission to get acquainted with the Soviet institutions engaged in the campaign against infectious diseases.

J. B. S. HALDANE AND THE UNIVERSITY OF CAMBRIDGE[1]

MR. J. B. S. HALDANE, of Trinity College, has been deprived of his office of reader in bio-chemistry in the University of Cambridge.

Mr. Haldane was the co-respondent in an action in the divorce court, in which the verdict went against him. He was subsequently arraigned before the Sex Viri, who reported that Mr. Haldane had been guilty of "gross immorality" within the meaning of the statute, and that he was, therefore, deprived of his office of reader in bio-chemistry. Among those who signed this report were the vice-chancellor (Dr. A. C. Seward), the master of Corpus Christi (the Rev. Dr. E. C. Pearce), Lord Chalmers, and the master of Trinity Hall (Dr. Henry Bond).

The announcement of the decree has just appeared

[1] From the London *Times*.

in the Cambridge University *Reporter* in the following terms:

Whereas the proceedings in the Probate, Divorce and Admiralty Division of the High Court of Justice, Mr. J. B. S. Haldane, reader in bio-chemistry at this university, has been pronounced by the verdict of a jury to have been guilty of adultery with the wife of Mr. John McLeod Burghes, and whereas Mr. J. B. S. Haldane, on appearing before us on the 6th day of November, 1925, admitted having committed adultery, now therefore, the vice-chancellor of the University of Cambridge and the six persons elected and acting in accordance with Chapter 7 of Statute A of that university, do hereby, in pursuance of powers conferred upon us by Statute B, Chapter 11, Section 7, of the said university, unanimously adjudge (1) that Mr. J. B. S. Haldane has been guilty of gross immorality within the meaning of the said Statute B, Chapter 11, Section 7; (2) that Mr. J. B. S. Haldane be therefore, and he is hereby, deprived of his office of reader in bio-chemistry.

Mr. Haldane has the right of appeal from the Sex Viri to a court of five judges delegate nominated by the Council of the Senate, and elected by grace. It is announced that appeal has been made to this court.

THE ELLA SACHS PLOTZ FOUNDATION FOR THE ADVANCEMENT OF SCIENTIFIC INVESTIGATION

THE Ella Sachs Plotz Foundation for the Advancement of Scientific Investigation was established in 1923, and during the first year of its existence the trustee received thirty-two applications for assistance from investigators in the United States, Europe and Australia. Eight grants were made. There has been considerable increase in the number of applications for assistance this year, the total being fifty-three. Fourteen of the applications came from eleven different countries in Europe and Asia, and the remainder were from the United States. During the past year one grant for a period of three years has been continued, and thirteen new grants have been made by the trustees. The list of investigators and of the researches which have been aided is as follows:

1. Dr. L. H. Newburgh, University of Michigan, $1,250 a year for three years for a study of the production of chronic nephritis with high protein diets and amino acids.

2. Dr. Edward F. Adolph, zoological laboratory, The Johns Hopkins University, $200 for the purchase of equipment to be used in investigations of the process of secretion with special reference to the kidneys.

3. Dr. Francis G. Blake and Dr. W. O. Winternitz, Yale University School of Medicine, $1,000 for the purchase and care of animals to be used in the study of the experimental production of nephritis with the toxin of scarlet fever.

4. Professor Lion Blum, faculty of medicine of the University of Strasbourg, France, $500 for the purchase of apparatus to be used in a research on the blood in various pathological conditions.

5. Dr. Alan M. Chesney, Johns Hopkins Hospital, $1,000 for the purchase and care of animals and for other expenses of a research on syphilis.

6. Dr. Joseph Erlanger and Dr. Herbert T. Gasser, school of medicine, Washington University, $1,000 for the purchase of an apparatus designed to record undistorted single action currents in nerve, to be used in the investigation of the physiology of nerve.

7. Dr. Ludwig Hofbauer, 1st medical clinic, Vienna, Austria, $200 for animals and apparatus to be used in researches on the effect of pathological conditions of the respiratory system on the body as a whole.

8. Dr. Edward B. Krumbhaar, Philadelphia General Hospital, $250 for the purchase of animals to be used in an investigation of biological resistance to experimental malignant tumors.

9. Professor A. Lipschutz, director of The Physiological Institute, University of Dorpat, Estonia, $1,000 for animals and apparatus necessary in a research on the internal secretions of the sex glands.

10. Dr. William deB. MacNider, University of North Carolina, $1,700 a year for two years towards general expenses of a study of experimental chronic nephritis.

11. Professor C. R. Schlayer, Augustahospital, Berlin, $250 for apparatus to be used in studies of the permeability of tissue to X-rays.

12. Professor Frederic Verzar, director of the physiological laboratories, University of Debrecen, Hungary, $1,000 towards the expenses of investigation on avitaminosis and internal secretions.

13. Dr. Joseph C. Aub, Massachusetts General Hospital, $500 for supplies and equipment to be used in a study of the internal secretion of the adrenal cortex.

14. Dr. H. C. Bazett, department of physiology, University of Pennsylvania, $500 for the purchase of a galvanometer to be used in a research on skin and subcutaneous temperatures.

Applications for grants to be held during the year 1926–27 should be in the hands of the executive committee before May 15, and should be sent to the secretary, Dr. Francis W. Peabody, Boston City Hospital, Boston, Massachusetts.

RESEARCH IN CEREBRAL PHYSIOLOGY AT THE UNIVERSITY OF IOWA

A TWO years' program of research in cerebral physiology is being undertaken at the State University of Iowa under a grant of $60,000 from the Rockefeller Foundation. This work is an extension of that begun in 1925 by the State Psychopathic Hospital, which is an integral unit of the college of medicine of the university. The project is under the direction of Dr. Samuel T. Orton, director of the Iowa State Psychopathic Hospital and professor of

psychiatry in the State University of Iowa, and is being carried out by two groups of workers, a mobile clinical or field group and a scientific or laboratory group.

The function of the field unit is to hold psychiatric clinics in various of the smaller communities of the state where adequate psychiatric service has not been available, and to locate problem material, referring selected cases to the laboratory group for more intensive study. The field unit includes a psychiatrist who is in charge of the clinical examinations, a psychiatric social worker, a psychometrist and a secretary. It is thus equipped to offer consultation in cases of true mental disease and in medico-legal cases of psychiatric bearing, in cases of social maladjustment arising from mental factors and in problems of behavior and intellectual development in school children.

The laboratory unit consists of the director, an executive assistant who also serves as field organizer for the mobile unit, an associate in neuro-anatomy and neuro-pathology, an assistant in neuro-physiology, a teacher to work on experimental methods for correction of special disabilities, a research associate of the National Research Council and a secretary. With the exception of the director all these collaborators are to give their full time to the research program.

The general plan is that of an attack on some of the problems of cerebral dominance through the study of cases of special disabilities in reading, writing and speech, by an analysis of the literature of cerebral anatomy and pathology and by animal experiment.

AMERICAN SCHOOL OF PREHISTORIC RESEARCH

THE American School of Prehistoric Research, of which Professor George Grant MacCurdy, of Yale University, is director, was incorporated recently under the laws of the District of Columbia. Its by-laws provide that the Archaeological Institute of America, the American Anthropological Association, and Section H—Anthropology—of the American Association for the Advancement of Science, shall be represented on its board of trustees.

The purpose of the school is to train students, to assist them as well as other investigators in the prosecution of their work in the field of prehistoric research, and to enrich museums, both in America and in the lands where researches are conducted with the material results of exploration. The board of trustees is authorized to maintain fellowships, instructorships, instruction and research stations, publications, as well as to raise funds for the support of the same and for the endowment of the school; it collaborates with universities and other scientific organizations both at home and abroad in the advancement of our knowledge of man's cultural and physical evolution.

During the five years of the school's existence, forty-two students have been enrolled, some on full time and others on part time. The students have come from thirteen states (Colorado, Connecticut, Georgia, Illinois, Massachusetts, Michigan, Missouri, New York, Ohio, Oregon, Pennsylvania, Tennessee and Wisconsin), the District of Columbia, and three foreign countries (Australia, England and Mexico).

The sixth summer session of the school will open in London on June 25. The program is so arranged as to combine lectures and work in museums with excursions and actual experience in excavating. In addition to the lectures by the director, conferences will be given on special topics by a number of distinguished specialists.

Dr. MacCurdy will represent the Smithsonian Institution and Yale University as well as the School at the Archaeological Congress to be held in Syria and Palestine from April 2 to 23.

SCIENTIFIC NOTES AND NEWS

ARTHUR L. FLAGG, consulting engineer of Phoenix, Arizona, has been elected president of the southwestern division of the American Association for the Advancement of Science. Dr. Forrest Shreve, of the Carnegie Institution, was elected vice-president and Dr. D. S. Robbins, of the New Mexico Mechanical and Agricultural College, a member of the executive committee.

PROFESSOR EDWARD S. DANA, of Yale University, has been elected honorary president of the Mineralogical Society of America for life.

DR. CHARLES F. MARVIN, chief of the United States Weather Bureau, has had conferred upon him the decoration of Knight of the first class of the Royal Order of St. Olav by the King of Norway.

DR. J. C. ARTHUR, professor emeritus of botany at Purdue University, has been made an honorary member of the Russian Botanical Society, in the section of mycology, in recognition of his researches on the rust-fungi.

THE Institution of Mining and Metallurgy has awarded the premium of the Consolidated Gold Fields of South Africa, Ltd., amounting to forty guineas, to the paper by Dr. H. C. Boydell, of the Massachusetts Institute of Technology, on "The Rôle of Colloidal Solutions in the Formation of Mineral Deposits." This premium, which is awarded annually, will be presented at the annual general meeting of the institution on April 15.

THE Botanical Society of America, at its recent meeting in Kansas City, elected the following corresponding members: Professors A. Engler and K. Correns, of the University of Berlin; Professor C. Sauvageau, of the University of Bordeaux; S. Nawaschin and Professor R. Willstätter, of the University of Munich. Previously elected corresponding members are Professors V. H. Blackman, F. C. Bower, Hugo De Vries, K. von Goebel and A. C. Seward.

M. CAMILLE MATIGNON, professor of mineralogical chemistry at the University of Paris, has been elected a member of the French Academy of Sciences in the section of chemistry.

THE nomination of Professors T. Madsen and P. Teissier as honorary foreign members of the Royal Academy of Medicine of Belgium has been approved by royal decree.

THE title of emeritus professor is to be conferred on Sir John Cadman, lately professor of mining, on his resigning his appointment as honorary professor and adviser to the mining department at the University of Birmingham.

DR. CHARLES H. MAYO, of Rochester, Minn., a member of the board of regents of the American College of Surgeons, was given a dinner on March 3, by 175 members of the college from Maryland, Virginia, West Virginia and the District of Columbia who were in attendance at the district meeting of the organization in Cumberland.

DR. LOUIS I. HARRIS, New York Commissioner of Health, was the guest of honor at a dinner in the Hotel Commodore, New York, on March 5. Among the speakers, besides Commissioner Harris, were Dr. James Alexander Miller, president of the New York Tuberculosis and Health Association; Dr. Livingston Farrand, president of Cornell University; Dr. C.-E. A. Winslow, of Yale University, and Dr. S. S. Goldwater, director of Mount Sinai Hospital.

THE Syracuse Academy of Medicine gave a dinner on February 2 in honor of Dr. Robert A. Hatcher, professor of pharmacology at the Cornell University Medical College, preceding an address by Dr. Hatcher on "The Relation of Pharmacology to Rational Therapeutics."

DR. A. J. CARLSON, professor of physiology at the University of Chicago, has been elected to succeed Dr. Julius Stieglitz (resigned) on the council on pharmacy and chemistry of the American Medical Association.

GEORGE S. DAVISON has been elected president of the American Society of Civil Engineers for the year 1926.

AT a meeting of the American Conference on Hospital Service, Chicago, February 17, Dr. Frank Billings, Chicago, was elected *honorary president;* Dr. Sigsmund S. Goldwater, New York, *president;* Dr. Ralph B. Seem, Chicago, and Mrs. Carl H. Davis, Milwaukee, *vice-presidents,* and Dr. Harry E. Mock, Chicago, *treasurer.*

DR. WILLIAM K. GREGORY, professor of vertebrate paleontology at Columbia University, was recently appointed curator of fishes at the American Museum of Natural History.

DR. ELLERY H. HARVEY, of Madison, Wisconsin, has joined the research staff of Swift and Company, Chicago.

PROFESSOR S. F. ASHBY has resigned his position as mycologist and bacteriologist at the Imperial College of Tropical Agriculture, Trinidad, to accept the post of mycologist to the Imperial Bureau of Mycology with headquarters at Kew.

F. DEBENHAM, fellow and tutor of Gonville and Caius College, University of Cambridge, has been appointed director of the newly formed Polar Research Institute at the university.

DR. DONALD MCKAY MORRISON has been appointed to the Canadian fellowship of the Ramsay Memorial Fellowship Trust and will study at the University of Cambridge.

THE Swarthmore College expedition, which went to Sumatra to observe the total eclipse of the sun in January, arrived in Boston on March 7. The party included Professor John A. Miller, Dr. H. D. Curtis, Professor Ross W. Marriott and Mr. D. B. McLaughlin.

DR. ROBERT CUSHMAN MURPHY, assistant director of the American Museum of Natural History, with Jesse Metcalf, will sail on March 20 for the island of Komodo, Dutch East Indies, where they will collect reptiles.

DR. HARRY N. HOLMES, head of the department of chemistry of Oberlin College, who is in Europe during the current session, recently spent two weeks in Italy. In Milan he visited Professor Bruni, of the Royal Polytechnic School, and at Turin gave a lecture on "Colloid Chemistry" before the chemical staffs of the university, the School of Mines and the Royal Engineering School. He also discussed his colloid research work with a small group of the faculty of the University of Rome. He has been invited to deliver lectures in March at the Universities of Leyden and Utrecht.

DR. G. J. HUCKER, associate bacteriologist at the New York State Agricultural Experiment Station,

has been granted a travel fellowship by the International Educational Board for a year's travel and study in Europe. Dr. Hucker will sail in August and plans to devote his time to a further study of the Micrococci, particularly the nomenclature and physiology of the group, in association with Professor St. John-Brooks at Lister Institute, London; Professor Barthel at Stockholm, and Professor Orla-Jensen at Copenhagen.

DR. JOHN E. GUBERLET has a leave of absence from the zoology department of the University of Washington and is spending some months at the University of Hawaii, Honolulu, where he is acting head of the department of zoology and is doing research work on animal parasites. Dr. C. H. Edmondson, permanent head of the department, is making a scientific tour around the world.

DR. J. W. GIDLEY, of the U. S. Bureau of Ethnology, left Washington on February 16 to renew his studies of remains of ancient man at Melbourne, Florida.

PROFESSOR ERNEST COHEN, director of the Van't Hoff Laboratories, University of Utrecht, Holland, will give advanced courses in physical chemistry at the University of Chicago during the first term of the summer quarter. Professor Cohen is president of the International Union of Pure and Applied Chemistry which will meet in the United States in September.

PROFESSOR J. N. BRØNSTED, of Copenhagen, will be visiting professor of chemistry at Yale University during the first term of the university year 1926–27.

DR. MAYNARD M. METCALF, of the Johns Hopkins University, who expected to return about March 1 from his six months biological research trip to South America, recently made an address at a meeting of the Rotary Club at Ancon, Panama, Canal Zone.

DR. HENRY C. SHERMAN, professor of chemistry, Columbia University, lectured on "Recent Advances in the Chemistry of Nutrition," at the University of Delaware, on February 16; to the Science Teachers Association of Washington, on February 17; at the University of Virginia, February 18, and to the Richmond section of the American Chemical Society, February 19. Dr. Sherman also lectured on "Chemistry as a Profession" at the Vassar College Vocational Conference on February 27 at Poughkeepsie.

DR. GEORGE C. SOUTHWORTH, of the American Telephone and Telegraph Co., addressed a joint meeting of the Washington Academy of Sciences and the American Institute of Electrical Engineers (Washington Section) in Washington on March 9, on the subject of "Some Interesting Things about Telephone Transmission."

DR. T. G. DELBRIDGE, supervisor of the process division of the Atlantic Refining Company, will give a lecture before the Franklin Institute, on March 25, on the "Cracking of Petroleum."

ON February 27, Dr. Roy W. Miner, curator of the department of lower invertebrates in the American Museum of Natural History, New York, delivered an address to the Royal Canadian Institute on the subject "Hunting Corals on the Bottom of the Sea."

PROFESSOR KNUD FABER, of the University of Copenhagen, lectured under the auspices of the Herter Lecture Fund, on February 18, at the Johns Hopkins Hospital, Baltimore, on "Disturbances of Blood Sugar Regulation."

IN celebration of the centenary of the death of Stanislas Staszic, scientific worker, business man and statesman, the Polish government has restored to the Warsaw Scientific Society the palace presented to that body by Staszic, but afterwards confiscated by the Russians.

ANNOUNCEMENT has been made of the establishment of the "Edward Livingston Coster Memorial Library" of railroad engineering volumes at the Pennsylvania State College library, the gift of the widow of the late Edward Livingston Coster.

DR. FREDERICK HARVEY BLODGETT (Ph.D. Johns Hopkins 1910), who recently joined the biology department of Washington Square College, New York University, died on March 4.

DR. ALBERT H. TUTTLE, Boston surgeon, formerly instructor in entomology at the Bussey Institute of Harvard University, died on March 1.

WILLIAM J. GREEN, for nearly forty years horticulturist at the Ohio Experiment Station and one of the pioneers in horticultural investigation, died recently, aged seventy-six years.

B. N. PEACH, F.R.S., formerly district geologist on H.M. Geological Survey, England, died on January 29, aged eighty-three years.

DR. M. B. R. SWANN, fellow of Caius College and demonstrator in pathology in the University of Cambridge, died from blood poisoning contracted during a *post-mortem* examination, on February 16, aged thirty-two years.

PROFESSOR D. S. CAPPER, formerly professor of engineering at King's College, London, died on February 12, aged sixty-one years.

PROFESSOR EDUARD HACKEL, the eminent Austrian agrostologist, died at his home in Attersee, on February 17, at the age of seventy-six years. Professor Hackel published numerous works upon the taxonomy of grasses, the best known of which is his monograph

on the Andropogoneae (1889). His extensive herbarium was acquired several years ago by the Natural History Museum of Vienna.

DR. EMILE ROMANN, formerly professor of agriculture and agricultural chemistry at the University of Munich and honorary president of the International Congress of Agriculture, has died, at the age of seventy-five years.

DR. N. HJALMAR NILSSON, for thirty-five years head of the Swedish Seed Association and director of the experiment station of that association at Svalöf, recently died, at the age of sixty-nine years.

THE United States Civil Service Commission announces the following open competitive examinations, receipt of applications for which close on the dates indicated: April 13, associate engineer (electrical, mechanical or signal) at a salary of $3,000; assistant engineer (electrical, mechanical or signal) at a salary of $2,400; April 17, junior engineer at a salary of $1,860; junior metallurgist at a salary of $1,860; junior chemical engineer at a salary of $1,860; April 20, research physicist (radio) at a salary of $5,400; May 1, junior chemist at a salary of $1,860; assistant scientific aid at a salary of $1,500; junior scientific aid at a salary of $1,320.

THE seventh annual meeting of the Southwestern Division of the American Association for the Advancement of Science was held at Phoenix, Arizona, during the week of February 15. It was the most successful meeting held by the division and was more largely attended than any previous meeting, there being over two hundred registered. A full report of the meeting will appear in SCIENCE shortly. The eighth annual meeting of the division is to be held in 1927 at Santa Fe, New Mexico, at a date to be determined later.

THE French Association for the Advancement of Science will celebrate its jubilee this year at the annual meeting to be held at Lyons. An international exhibition for the advancement of science, to include all branches of science and its applications, is being organized for July 24 to August 1. M. Pilon, 23 rue Casimir Perier, Paris, has been appointed commissaire général of the exhibition.

THE twelfth International Physiological Congress will meet at Stockholm from August 3 to 6, 1926. The address of the secretary is Dr. G. Liljestrand, Karolinska Institut, Stockholm, Sweden. Professor J. E. Johansson is to preside at the meeting.

THE thirty-sixth annual meeting of the Ohio Academy of Science will be held at the Ohio State University, Columbus, Ohio, on April 9 and 10. Requests for information regarding the meeting, hotel accommodations or other service will receive prompt consideration if addressed to the chairman of the local committee, Dr. Dwight M. Delong, or to the secretary, Dr. William H. Alexander, 16 East Broad Street, Columbus, Ohio.

THE thirty-first annual meeting of the Michigan Academy of Science, Arts and Letters will be held at Ann Arbor from March 31 to April 2. The presidential address will be given by Charles H. Cooley on "The Roots of Social Knowledge."

THE American Society of Mammalogists will hold its eighth annual meeting at the American Museum of Natural History on April 28, 29 and 30. Special exhibits will be arranged for the mammalogists in the halls and laboratories of the departments of mammals, vertebrate paleontology and comparative anatomy. Papers will be given in morning and afternoon sessions during the conference.

THE Association of Chemical Equipment Manufacturers will hold its second annual exposition from May 10 to 15 in Cleveland. This exposition is managed by a national trade association, the aggregate worth of which is close to $1,000,000,000.

PUBLIC lectures for the months of March and April are being given at the New York Botanical Garden on Saturday afternoons at 3:30 as follows: March 6, "Poetry in the Names of Flowers," Miss Laura Lee Rogers; March 13, "Plant Arrow-Poisons: Their Sources, Preparation and Effects," Dr. Ralph H. Cheney; March 20, "Wild Flowers and their Insect Visitors," Professor Oliver P. Medsger; March 27, "Botanical Travel in Peru and Chile," Dr. Francis W. Pennell; April 3, "The Flowers of the Prize Garden," Mr. Kenneth R. Boynton; April 10, "A Historical Review of the Study of Fossil Plants," Dr. Arthur Hollick; April 17, "The Desert Vegetation of the Southwest," Professor John W. Harshberger; April 24, "Narcissi, or Daffodils," Mrs. Wheeler H. Peckham.

THE Sigma Xi Club of Carleton College, Northfield, Minn., is presenting the following program during the current school year: October 22, Edward A. Fath, "What is a Star?" November 20, Edward Bartow, "Progress in Water Purification and Sewage Disposal." December 10, Herbert C. Wilson, "The Motions of the Stars." February 12, Wm. O. Beal, "Star Clusters and Nebulae." March 18, Charles A. Culver, "Technical Problems involved in Radio Broadcasting." April 22, Curvin H. Gingrich, "The Solar System." May 20, Neil S. Dungay, "Recent Researches on Scarlet Fever."

THE National Research Council announces that a limited number of fellowships will be available for

the year 1926–27 for qualified candidates wishing to prepare for work in the field of child study and parent education. Only candidates with the bachelor's degree from an accredited institution who have had previous preparation and experience in a related field of work will be considered. Applications must be received by April 5. Blanks and information may be had from the Division of Anthropology and Psychology, National Research Council, Washington, D. C.

ANNOUNCEMENT has been made of the establishment of two annual prizes at Harvard University of $150 and $100 to be awarded to students in the college or graduate school of arts and sciences for the best thesis on subjects connected with the philosophy of William James. Provision for the award of these prizes during the next two years has been made by the gift of Edwin Det Bechtel, of New York City, one of Professor James's former students.

THE Mellon Institute of Industrial Research, University of Pittsburgh, has issued a list of the periodicals in the institutional library. According to this publication, which contains 287 entries, 214 journals are received current by the institute. Upon request, copies of this list will be sent gratis to chemical librarians and directors of research laboratories.

THE California Academy of Sciences recently sent supplies of top minnows (*Gambusia affinis*) to India, Philippine Islands and other places in the Far East, also to Papeete, Tahiti. The academy finds that these minnows multiply as rapidly under aquarium conditions in Golden Gate Park as they do in their native pools; the academy has therefore been able to supply from the Steinhardt Aquarium colonies of minnows to many parts of the world.

THE trustees of the Carnegie Fund, England, have promised £1,000 to the library fund of the British Optical Association, if its members succeed in raising a similar sum within two years.

AN annual prize for surgery, which will be named after Dr. Archibald Young, professor of surgery at Glasgow University, has been instituted by the Lisbon Faculty of Medicine.

It is proposed to form, at an early date, an Irish Center of the Institute of Wireless Technology, with the objects of promoting the general advancement of radio technology and procuring the recognition of the status of the institute by the government, by public and local authorities and other bodies in the country.

THE Society of Neurology at Paris announces that the surplus left after the celebration of the Charcot centennial last June has been invested to endow a triennial prize of 4,000 francs. It is not open to competition. The society will select the beneficiary and give him a topic for research, clinical, anatomic or experimental, in the domain of neurology, on which he will report in three years at the latest.

Nature states that, by the will of the Right Hon. Stephen Ronan, lately Lord Justice of Appeals in Ireland, who died on October 3 last, and left personal estate in England and the Irish Free State valued for probate at £83,907, the Medical Research Council will receive considerable benefits. After private bequests amounting to nearly £9,000 and legacies to Irish charities, the residue is left to the Medical Research Council to be applied in assisting and promoting scientific research as the council may think best, "but without limiting their discretion, I would wish that special attention should be given to the relief, cure and prevention of physical pain by physical means."

THE committee of the American Association for the Advancement of Science on the place of the sciences in education held an open meeting in Kansas City during the last convocation week. The four papers constituting the program were published in *School and Society* for February 20. These papers were presented by Professors Caldwell, Livingston, Pupin and Dr. Slosson, and may be had in one pamphlet free by addressing the chairman of the committee on place of sciences in education, 425 West 123d St., New York City.

EVOLUTION will be an important topic at the twenty-seventh summer session of Columbia University, which will begin on July 6 and end on August 13. In public lectures and in classroom and laboratory courses an attempt will be made to correct popular misconceptions of the theory of evolution. Public lectures will be given on July 15, 22 and 29, and on August 4. Professor Harlow Shapley, of Harvard University, will discuss "The Evolution of the Stars"; Professor Richard S. Lull, of Yale University, "The Evolution of the Earth"; Henry E. Crampton, of Columbia University, "The Evolution of Plants and Animals"; and Professor Edward L. Thorndike, of Columbia University, "The Evolution of Intelligence."

A CORRESPONDENT sends us the advertisement of a furniture company, of Jackson, Miss., which begins as follows: "No such varmint living as an infidel or evolutionist. Listen to their mournful cries when death stares them square in the face. We do not believe in such. But we do believe in high quality and low prices. A beautiful two-piece Overstuffed Upholstered in Mohair with reversible cushions for $185.00 on terms or 25 per cent. off for cash."

AT the Washington meeting of the American Asso-

ciation for the Advancement of Science, Section M (engineering) proposed a resolution which, on April 26, 1925, was adopted by the executive committee in the following form: "Resolved, that the American Association for the Advancement of Science heartily approves the establishment of a National Museum of Engineering and Industry, to be located in Washington." On October 25, 1925, the executive committee appointed the president of the association, Dr. Michael I. Pupin, as its representative on the board. The complete list of the board is given in the number of SCIENCE for February 12.

THE biological laboratories of the United States Bureau of Fisheries, located at Woods Hole, Mass., Beaufort, N. C., and Fairport, Iowa, will open on June 21, and are expected to remain in active operation until about September 15. A limited number of research rooms and tables will, as usual, be available to those qualified to conduct investigations in the various branches of marine and fresh-water biology. Owing to the increased demand for accommodations at the Woods Hole laboratory it has become necessary to make a more careful selection of those who will be granted the privileges of the laboratory, and preference will be given to those investigators who are working along lines of especial interest to the Bureau of Fisheries, and who have shown ability for energetic and productive research. The opportunities and facilities of Woods Hole and Beaufort are well known. At Fairport there is a new and well-equipped laboratory, with the necessary collecting apparatus, and ponds and tanks. Both river water and filtered water are provided. Those desiring to have the use of tables and other facilities at these laboratories may communicate with Henry O'Malley, Commissioner of Fisheries, Washington, D. C.

IN the paper by Callie Hull and Clarence J. West on "Doctorates Conferred in the Sciences" printed in SCIENCE for December 25, the Iowa State College of Agriculture and Mechanic Arts is credited with having given no doctorates in the sciences from 1916 to 1923, whereas it should have been stated that thirteen doctorates were conferred during this period. The table of doctorates conferred shows nine for the year 1924 and twelve for the year 1925. The correct figures are eleven and thirteen, respectively.

UNIVERSITY AND EDUCATIONAL NOTES

WESLEYAN UNIVERSITY has received from an anonymous donor the sum of $40,000 for research work in chemistry, to be known as the Wilbur Olin Atwater Fund, in honor of the late Dr. Atwater, who was professor of chemistry at Wesleyan.

CORNELL UNIVERSITY has received a bequest of $25,000 for the use of the college of agriculture from the estate of the late Gustav Ulbricht, who died on February 19, 1924.

DR. HAROLD C. M. MORSE has been appointed assistant professor of mathematics at Harvard University.

EARL THORPE SCOTT, assistant plant pathologist of the Missouri Agricultural Experiment Station, has been promoted to the rank of assistant professor of botany.

ROLAND M. CHASE has been appointed professor of civil engineering and mathematics at the Alaska Agricultural College and School of Mines, Fairbanks, Alaska.

J. AUSTIN BURROWS, of the chemistry department of Iowa State College, has been appointed assistant professor in inorganic chemistry at the University of North Dakota, University Station, Grand Forks, North Dakota.

PROFESSOR C. A. LOVATT EVANS has been appointed to the Jodrell chair of physiology at University College, London, to take the place of Dr. A. V. Hill, who recently resigned to take a Foulerton chair of the Royal Society, tenable at the college.

DR. RATHERY has been elected professor of experimental pathology, and Dr. Lemierre professor of bacteriology, in the Paris Faculty of Medicine.

PROFESSOR FILIPPO BOTTAZZI, director of the institute of physiology and biological chemistry, has been selected to succeed Professor Zambonini in the rectorship of the University of Naples.

DISCUSSION AND CORRESPONDENCE

BLACK CHAFF OF WHEAT IN RUSSIA

IN 1917 I described a bacterial disease of wheat from our middle west under the title: "A New Disease of Wheat" (*Jour. Agr. Res.* X, 51). That year I had fifteen persons at work upon it with three collectors in the field. The same year I contributed two notes to the mimeographed Plant Disease Bulletin (issued by the Plant Disease Survey, Bureau of Plant Industry, at intervals) in which I further described the disease, mapped its distribution and speculated on its origin. From its restricted distribution (states west of the Mississippi, where much Russian hard wheat had been introduced), from its very infectious nature (as indicated by its occurrence in many localities and by our very successful inoculation experi-

ments), and from the fact that it is transmitted on the seed from one crop to the next, I assumed it to be a recent importation. I confined myself to saying only that much in 1917, but in 1920 I stated textually:

> I think, for example, that our black chaff of wheat is an importation from Russia. At least it should be searched for in that country. It was not observed in the wheat region west of the Mississippi River until after numerous importations of Russian wheats.[1]

Word now comes from Russia[2] that this disease was observed in many localities in Russia in 1924. It occurred also in 1923 in the Crimea and he has found it on Russian wheat collected as long ago as 1916, Rjovzi, Government of Poltawa, and in wheat collected in 1910 in the province of Mohilew, Government of Elis-a-vetpol.

I quote from a translation as follows:

> In 1924 the disease was noticed in the Kuban district, in the Don district, in the Governments of Charkow, Kiew and Woronesh. . . . There remains no doubt whatsoever that this form of bacteriosis exists in Russia, where it is evidently widely distributed for a long time.

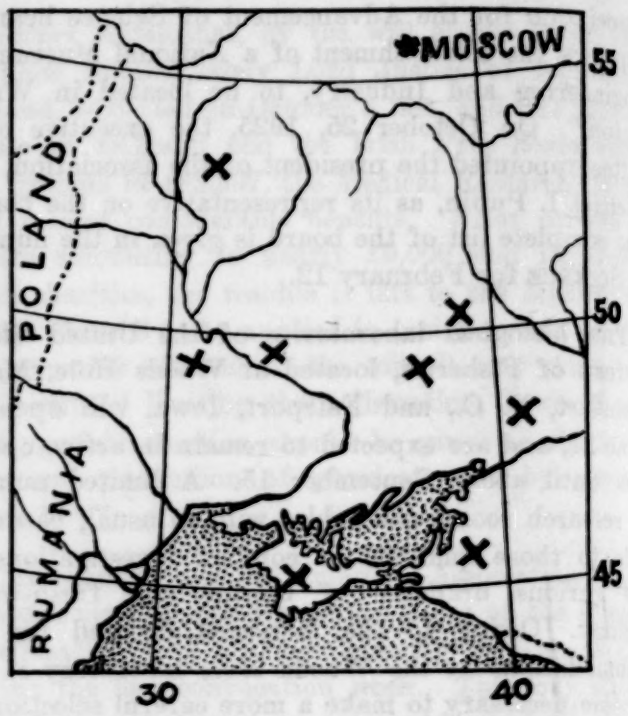
Janczewsky thinks he has also observed it on wheat collected in Sunpan, China, in 1893.

A. Kusmenko, in a letter to K. S. Flaksberger, cited by Dr. Janczewsky, writes as follows:

> This summer, while working in the Ivanow Agricultural Experiment and Breeding Station, where there is a large collection of wheats, I have observed an exceedingly interesting fact of pigmentation of many winter wheats which I wish to communicate to you.
>
> The pigmentation (coloring of glumes and beards into dark color) was exclusively observed on red wheats and consisted in that the entire (or part of the) beard or of the glume (or both together) blackened on the given ear. This phenomenon is observed for the first time on wheats which never had it before, even in individual cases, while this year it represents a mass occurrence with many wheats. . . . On closer investigation, pigmentation was also found on the typical white, beardless wheat—"Rakooka" No. 13/676 of the Charkow Agricultural Experimental Station, whereby its glumes were heavily colored (black).

The bacteria have been found not only in the glumes but also in the kernels.

The black chaff, therefore, occurs, according to Professor Janczewsky, the well-known Russian mycologist, in eight widely separated localities in southwest Russia, to wit, in the provinces of Crimea, Kuban, Don, Charkow, Woronesh, Poltawa, Kiew and Mohilew, stretching over an area of approximately ten degrees of latitude and longitude. I append an outline map of southwest Russia with the localities marked by a cross. From this it will be seen that the disease occurs in that part of Russia explored by Mr. Mark Alfred Carleton for the U. S. Department of Agriculture in 1898 and again in 1900, and from which under his direction we imported large quantities of hard wheats for planting in our semi-arid west. The disease, according to Dr. Janczewsky, undoubtedly occurs in other parts of Russia.



I have always considered Mr. Carleton's work to be the most far-reaching and practical piece of work ever done by the Bureau of Plant Industry, since in a district in our west stretching from Texas to North Dakota and covering several degrees of longitude, through his energy and ability, we now grow annually 100,000,000 bushels of the Russian hard wheats, where previously we did not grow any.

I write this not to condemn Mr. Carleton but only to point out that, if our government were as intelligent as it ought to be (few governments have much foresight), we should now have agents scouring the whole world studying all sorts of crops and crop diseases so that in future when we import valuable ornamental plants and food plants we may do so without at the same time bringing in their parasites. Had we known of this Russian wheat disease in 1889 we should have imported the Russian hard wheats more slowly and grown the plants in quarantine first and so have avoided introducing the parasite along with the grain. In similar ways we might have avoided the introduction of a dozen very destructive parasites which have come to us from the old

[1] "Bacterial Diseases of Plants," W. B. Saunders Co., Philadelphia and London, 1920, p. 66.

[2] Janczewsky, *Bull. Applied Botany and Plant Breeding*, Vol. 14, No. 1, pp. 377–385.

world in the last three decades. The United States, even at the present time, is very derelict in making explorations in foreign countries for the benefit of its citizens and the conservation of its industries, but if we would lead the world, we must change our policy. Japan is the only country thoroughly awake to the need of foreign exploration. Her scholars are in every quarter of the globe, dozens of them, picking up every grain of information possible for use in the mother country. It is much to be regretted that we have not already adopted the same far-sighted and commendable policy.

ERWIN F. SMITH

BUREAU OF PLANT INDUSTRY,
U. S. DEPT. OF AGRICULTURE,
WASHINGTON, D. C.

A SEISMOLOGICAL NOTE

THERE seems to be rather widespread misunderstanding as to the way in which the velocity of seismic waves varies with the density of the medium in which they travel.

From the theory of elasticity it is developed that the velocity of longitudinal waves in an elastic medium is $\sqrt{\frac{\lambda+2\mu}{\varrho}}$ where λ is Lame's compression constant, μ is the rigidity and ϱ is the density. The velocity of transverse waves is $\sqrt{\frac{\mu}{\varrho}}$. For surface waves of the Rayleigh type the velocity is $0.92\sqrt{\frac{\mu}{\varrho}}$ where Poisson's constant is taken as 0.25.[1] In the Rayleigh wave the particle vibrates in an ellipse in a vertical plane which contains the direction of propagation. Uller[2] has also found theoretically a value for the velocity of surface waves which varies inversely with the square root of the density.

Now Angenheister[3] found a greater velocity under the Pacific than under Asia for both types of surface waves, the L and M groups. He also found for nearby earthquakes a greater velocity for the preliminary groups for an under-sea path. Tams[4] and Visser[5] have also found the L group to have a greater velocity for sub-oceanic paths. Gutenberg[6] found for the Chilean earthquake a greater velocity in the M group for a Pacific path than for a path under South America, the Atlantic and Europe. He found the velocity of the L group to be independent of path. But it is important to notice that the phase of very long waves identified by him as the L group, and checked by the writer, are of greater velocity than the group which has usually been identified as L. Thus it is apparently not the group identified as L by other investigators, as Gutenberg suggests. The new velocity is 4.35 to 4.4 $\frac{\text{km}}{\text{sec}}$. For oceanic paths Angenheister found a still greater velocity for L but a lesser velocity for continental paths.

It seems established then that for at least part of the surface waves the velocity under the Pacific is greater than the velocity under continents.

Some writers have cited this increase of velocity under the Pacific as evidence that the density is there greater than under the continents.[7]

But from this increase alone the conclusion would be that of a less density beneath the ocean, since the velocity varies inversely with the square root of the density. It is only when we compare with gravitational measurements which indicate a greater density for ocean bottoms that we are forced to conclude that the greater velocity of seismic waves beneath the Pacific should be explained, as we explain the velocity increase with depth in the earth, by an increase in the elastic constants λ and μ which more than compensates for the increase in density.[8]

Thus we see that the increased velocity of seismic waves beneath the Pacific can not be cited as an evidence of greater density beneath oceans than beneath continents.

PERRY BYERLY

UNIVERSITY OF CALIFORNIA

THE TREATMENT OF WART DISEASE OF POTATOES WITH SULPHUR

IN the "Annals of Applied Biology," XII, 2, 1925, a paper was published by Roach, Glynne, Brierley and Crowther entitled "Experiments on the Control of Wart Disease of Potatoes by Soil Treatment with Particular Reference to the Use of Sulphur." In 1922

[1] G. Angenheister, "A Study of Pacific Earthquakes," *New Zealand Journal of Science and Technology*, Vol. 4, No. 5, 1921; also J. H. Jeans, "The Propagation of Earthquake Waves," *Proc.* Royal Soc. London, A, Vol. 102, 1923.

[2] Karl Uller, *Annalen der Physik*, Folge 4, 56, 1918, S. 463.

[3] *Op. cit.*

[4] E. Tams, *Centralblatt für Mineralogie, Geologie und Paläontologie*, Jahrgang, 1921, 2, S. 51.

[5] S. W. Visser, "On the Distribution of Earthquakes," Batavia, 1921.

[6] "Das Erdbeben in der chilenischen Provinz Atacama am 10. November, 1922," Veröffentlichungen der Reichsanstalt für Erdbebenforschung in Jena, heft 3, 1924.

[7] N. H. Heck, "Earthquakes of 1925," Bulletin of the Seismological Society of America, Vol. 15, No. 2, June, 1925, page 107; also Alfred Wegener, "The Origin of Continents and Oceans," English translation, Methuen and Co., Ltd., London, 1925, page 35.

[8] Angenheister, *op. cit.*

treatment of a light soil at Ormskirk with sulphur had given promising results, whilst in 1924 the amount of disease was reduced from 73 per cent. in untreated soil to 8 per cent. with an incorporation of 10 cwts of sulphur per acre and to less than 4 per cent. with 20 cwts sulphur per acre. On a heavy clay at Hatfield clean plots were obtained with an application of 40 cwts per acre. During the past season a larger experiment has been carried out to test these results and the deductions drawn from them and to determine whether the effect be a permanent one. As we have learned that experiments are being set up in other countries to test the efficacy of this sulphur treatment it is desirable that the discrepant results obtained by us this season should be known as soon as possible. In our test at Ormskirk the following arrangement was adopted.

Plot	Treatment
1	None.
2	15 cwts Sulphur per acre in Autumn.
3	10 " " " " " "
4	7½ " " " " " "
	7½ " " " " " Spring.
5	5 " " " " " Autumn.
	5 " " " " " Spring.
6	15 " " " " " "
7	10 " " " " " "
8	None.

In the untreated plots the plants grew well and were heavily warted. In the treated plots a first set of tubers planted in May and a second set planted in July almost entirely failed to grow. The surviving plants showed in all plots considerable amounts of wart disease, although much less than in the control areas. At Hatfield two tons of sulphur per acre was applied; the crop was damaged and a considerable amount of wart disease was present.

Unavoidable differences in the conditions (seasonal, manurial, etc.) under which the work was carried out in 1924 and in 1925 suggest certain explanations of these results, but we are not in a position to say that any one of them is correct. The results already published are not of course invalidated, but it is clear that the sulphur treatment can not, in the absence of further information on the soil and other factors involved, be regarded as a reliable method for freeing soil of the parasite causing wart disease. A more detailed account of our work will be published in the "Annals of Applied Biology," XIII, 2, 1926.

W. A. ROACH
WM. B. BRIERLEY

ROTHAMSTED EXPERIMENTAL STATION,
HARPENDEN

THE ATTITUDE OF THE ELECTRICAL RAILWAY COMPANIES ON ELECTROLYSIS

THE creed of the scientist and the engineer always has been to follow unbiasedly Truth, wherever she may lead, so that in solving any problem the engineer seeks to obtain the *whole* truth and then to act according to that truth.

However, certain incidents seem to show that there is a tendency not to follow literally this much respected and necessary ideal, where the problem being studied lies between directly interested parties. This is very regrettable, since a suppression of data, obtainable by one group but not by the other, necessarily inhibits the solution of any engineering or scientific problem. Moreover, such an attitude among men of science loses the confidence of the public and lessens their respect.

This departure from our creed has been forcibly brought to my attention by recent incidents which have occurred in Los Angeles in connection with studies on corrosion by stray current electrolysis.

The electrolytic conditions of Los Angeles have been and are such as to demand the attention of all metallic substructure owning companies and the electric railway companies, and therefore, engineers in general.

The Los Angeles Section of The American Institute of Electrical Engineers decided to have an academic paper on the general subject of "Electrolysis," presented at one of its monthly meetings. I was asked to prepare and present such a paper.

Upon hearing that the subject of electrolysis was to be presented locally and before it was known what was to be said, the engineering representatives of the electric railways held an indignation meeting, criticized the action of the local section of the A. I. E. E. in scheduling the subject of electrolysis and resolved not to attend the meeting.

I prepared copies of the address and presented them to the engineering representatives of both the railway companies and the public utility companies owning affectable structures so that they could prepare any discussion or raise any objections prior to the presentation of the paper. Up until the time of giving to the railway companies these copies of the proposed address, I knew nothing of the attitude that they had taken.

Upon learning their view of the matter, wishing to have the railways' representatives at the meeting and not desiring to jeopardize the cooperation between any of the parties, I offered to cancel all parts of the paper to which the railway companies objected. They had very few objections to the paper, but all parts that they did object to were cancelled. All reference to Los Angeles was omitted. However, the railway companies were not satisfied and

were not present at the meeting on January 5, 1926, when the paper was presented. The railway companies made it plain that what they objected to was any mention whatever of the subject of electrolysis. They offered as their excuse that the subject was a personal one and against the ethics of the American Institute of Electrical Engineers to study or discuss it. Evidently they forgot that the American Committee on Electrolysis, which published a unanimous report in 1921, was organized by and worked under the auspices of the American Institute of Electrical Engineers, and that on this committee were representatives of the American Electric Railway Association and the American Railway Engineering Association.

If the attitude herein presented is held by the electric railways of America, then the stray current electrolysis problem will have to be solved in the courts of law, which is not believed to be an ideal place to reach a satisfactory solution of any engineering or scientific problem.

I would like to learn the opinions of other engineers and scientists in regard to this matter.

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SCIENTIFIC BOOKS

J. C. Poggendorff's Biographisch-literarisches Handwörterbuch für Mathematik, Astronomie, Physik, Chemie und verwandte Wissenschaftsgebiete. Bd. V: 1904–1922. Redigiert von Professor Dr. P. WEINMEISTER. I. Abtlg.: A-K. Leipzig, Verlag Chemie, 1925.

FOR those who are engaged in scientific reference work "Poggendorff's Biographisch-literarisches Handwörterbuch zur Geschichte der exacten Wissenschaften . . . aller Völker und Zeiten" is an indispensable tool. The first two volumes were published in parts. The printing commenced in 1858 and the last part was issued in 1863. The third volume, covering the period from 1858 to 1882, was published in 1898 and the fourth volume, dealing with the period from 1883 to 1902/3, was published in 1904. The war and the war's aftermath caused a temporary discontinuance of this monumental undertaking, but we hail now with great satisfaction the appearance of the first part (A-K) of the fifth volume, which embraces the years 1904–1922.

The conception of this work originated with Johann Christian Poggendorff (1796–1877). As editor of the "Annalen der Physik und Chemie" (1824–1876), as author of "Lebenslinien zur Geschichte der exacten Wissenschaften" (1853), which contained the dates and main works of 150 scientists of the sixteenth to nineteenth century, as author of "Geschichte der Physik," edited after the author's death by W. Barentin and published in 1879, and as a scientist, who by his own inventions and investigations will never be forgotten, no one was better fitted for such a gigantic task. It took Poggendorff fifteen years of his spare time to complete the first two volumes. The third volume was edited by Dr. W. W. Feddersen and Professor A. J. von Oettingen, the fourth volume by von Oettingen alone, and the present volume by Professor T. Weinmeister. Of course a biographical work of such a character could not be compiled by one man and all the editors gratefully acknowledge the great help rendered them by many scholars in different countries.

The principles which guided the editors in their program are of the greatest importance. The title-pages of Volumes 1, 2 and 3, which are almost identical, say very plainly that the Biographisch-literarisches Handwörterbuch is only devoted to the representatives of the exact sciences and enumerates mathematicians, astronomers, physicists, geometricians, mineralogists, geologists, etc. In Volumes 3 and 4 after the geologists the geographers are named. On the title page of the fifth volume the words "exacte Wissenschaften" are omitted and the title reads "Biographisch-literarisches Handwörterbuch zur Mathematik, Astronomie, Physik, Chemie, und verwandte Wissenschaftsgebiete."

In the classification and history of sciences there were generally two divisions recognized: The exact sciences and the cultural sciences (Geisteswissenschaften). With the growth of our fields of knowledge and by a changed conception of different branches of science the term "exact sciences" has lost its old meaning and application. For this reason it seemed to be proper to replace the term "exact sciences" by the term "natural sciences," which is subdivided into "descriptive natural science" and "exact natural science." It was Virchow who said, "Every science is natural science." Taking this into consideration, let us see how Poggendorff and his successors in the editorship of the Handwörterbuch proceeded. In the introduction of the first volume Poggendorff states precisely that scientists whose works dealt with the living nature (lebende Natur) were excluded. This principle—very regrettable—eliminated anthropologists, biologists, botanists and zoologists. The editors of the third volume, Drs. Feddersen and von Oettingen, adhered to the same principle outlined by Poggendorff, although they had received some communications protesting against the undue preference of mathematicians, physicists, etc. On the other hand, they recognized the difficulties facing them in the

proper consideration of the allied sciences (Grenzgebiete). The single editor of the fourth volume (1883–1902/3), Dr. von Oettingen, has followed in the footsteps of his predecessors. In the fifth volume, edited by Dr. P. Weinmeister under the auspices of the Saxon Academy of Sciences in Leipzig and other academies, there are further limitations in the scope of the work. "To prevent the increase in the volume we have taken less consideration of the allied sciences." This policy of the editor and the academies which fathered the new volume is in my opinion a great mistake.

With the publication of Volume 5, the time had come not to limit but to widen the scope of the Handwörterbuch. Physical anthropology, biology (animal, botanical and human), comparative zoology, physiology, medical chemistry and entomology—these subjects should not have been omitted.

On the other hand, the new volume considered from a bibliographical standpoint deserves the highest praise and is a worthy successor to the previous volumes of this monumental work.

The policy of the previous editors to request scientists, especially those living outside of Germany, to prepare their own biographical statement was continued in this volume, without consideration of political boundaries. "Real science does not know political boundaries," says the editor. All the requests were answered and there will be found quite a number of original contributions by American, Belgian, English and French scholars.

In the preface of the fourth volume the editor recommends, in case the work should be continued, that for each branch of science to be dealt with an advisory board should be appointed to decide on the names to be included. It seemed that this suggestion was not carried out in the fifth volume. For the publication of future volumes I would like to recommend the election of such a board in each country, which would prevent regrettable omissions.

FELIX NEUMANN

ARMY MEDICAL LIBRARY,
WASHINGTON, D. C.

Eleventh Report of the Committee for the Investigation of Atmospheric Pollution. Observations in the Year ended March 31, 1925.

WE have in this report[1] a review of the work done by nineteen authorities cooperating in collecting data from forty-eight different stations. Forty-four of the forty-eight gave complete results.

Extensive tables give the deposits in grams per square dekameter for each month, or metric tons per hundred square kilometers.[2] It is noted first that Birmingham, farthest from the sea, has least chlorine both for the past year and in the general average for five years. A rather curious relation appears to exist between the amount of sulphate deposited from the air and the amount of total impurity. The greater the total impurity, the lower the percentage of sulphates. Also there appears to be an inverse relation between percentage of carbonaceous matter and sulphates.

The effect of wind on impurity is graphically brought out by plotting the mean values of the suspended matter for all observations at particular wind velocities against wind velocity in meters per second. The result is a curve, the equation of which is $I = \frac{0.55}{V} + 0.27$, I being the concentration in milligrams per cubic meter and V the velocity in meters per second.

It is remarkable that this curve indicates a variation of the concentration of impurity inversely as the first power of the wind and not as some higher power, which one would expect if there had been free lateral and vertical spread of the smoke as well as the stretching-out downwind. The actual results show what a profound effect wind has on concentration.

When the wind drops below one meter per second in winter, a smoke haze or fog in London appears almost inevitable under present conditions.

From some experiments made by Dr. Owens, assuming that the density of the particles is 1, and the average diameter 0.8 micron, the velocity of settlement will be 0.003 centimeters per second and the number falling on one square centimeter per minute should be 2880. Actual results are greatly in excess of the theoretical number, indicating that other factors are operative.

"London Particular," it appears, is found by a replacement of water particles by smoke and not by a dirtying of the condensed water by smoke.

A. MCADIE

BLUE HILL OBSERVATORY

SPECIAL ARTICLES

THE IONS OF INERT GASES AS CATALYSTS

IN SCIENCE of December 25, 1925, several gas reactions were reported in which we had found that the ions of certain inert gases, produced by radiating a mixture of inert and reactant gases with alpha rays,

[1] Abstracts of earlier reports are to be found in SCIENCE, August 8, 1924, June 2, 1922, April 22, 1921, and November 28, 1919.

[2] To convert metric tons per hundred square kilometers multiply by 0.0256 to get English tons per square mile; 0.09 to get pounds per acre.

accelerated the rate of reaction without themselves reacting. The ions of the inerts were found to contribute just as much to the reaction as if the same quantity of ionization had been produced on the reactants in addition to the amount actually produced on them by direct action of the alpha rays. In other words, the reaction is proportional to the total ionization, not to the ionization of reactants alone. It was pointed out that this relation could be readily accounted for by exchange of charge if the positive ion of the inert could take an electron from the reactant molecule, thus leaving the latter positively charged. Such a theory appeared the more plausible, since the inert gases employed up to that time had ionization potentials higher than those of the reactants.

The alternative possibility was also mentioned, that *addition* of reactant and inert, not *exchange of charge* might prove to be the case, and that these points could be tested by using inerts like xenon and krypton with potentials lower than those of the reactants. For clustering by addition, a high *positive* catalysis would be found, but a high *negative* one might be expected for exchange under the influence of ionization potential, assuming the inert ion itself to have no clustering power.

Dr. R. B. Moore kindly put at our disposal several cubic centimeters of xenon and of krypton, portions of the original gases which he had fractionated for atomic weight determination[1] in Sir Wm. Ramsay's laboratory. Each of these after thorough drying was given a vigorous treatment with metallic calcium at 475° C. to remove any impurities that might have been accidentally introduced since they were prepared. They were then used to mix separately with reactants, acetylene, cyanogen and hydrogen cyanide. The velocity of polymerization was manometrically determined in the mixtures under the alpha radiation of known amounts of radon. The results unambiguously showed catalysis in proportion to the krypton and xenon ions, just as for the other inerts, He, Ne and A. The order of ionization potentials for all gases concerned is: He (24.5); Ne (21.5); C_2N_2 (16.3); A (15.2); HCN (14.8); CO_2 (14.3); Kr (12.7); C_2H_2 (12.3) and Xe (10.9).[2]

Since in all cases the reactions were positively catalyzed in proportion to the specific ionization regardless of the ionization potential, it is evident that the latter is not a factor, and that clustering about both kinds of ions as reaction centers is indicated. Of course, this catalytic evidence does not show that exchange of charge may not take place physically in the direction predicted by ionization potential, but it does prove conclusively that such exchange even if occurring is probably simultaneous with addition and does not influence the rate of reaction, which is proportional to the total ionization regardless of whether it be on an inert or on a reactant.

Indirect evidence of clustering about an inert may be seen in the fact that a small fraction of xenon in all three gases, C_2H_2, C_2N_2, and HCN, was found to have been removed from the gas phase along with the precipitated polymer. The xenon was easily recovered quantitatively by heating to about 300° to 350° C. By integrating the separate ionizations of xenon and of acetylene over the entire course of the reaction, the amount of inert removed by occlusion was calculated on the assumption of complete coprecipitation of the inert with each ion cluster of which it formed the nucleus. The quantity so calculated was five times greater than that found, showing that quantitative occlusion does not result from clustering. If all instead of part of the clustering had taken place around xenon, the discrepancy becomes yet greater. Apparently the inerts of higher molecular weight are more readily occluded in the polymer, as might be expected, than those of lower. The case of CO_2 removal with C_2H_2 precipitation, previously mentioned (*loc. cit.*), is more probably accounted for in this way than through true chemical action, but does not support exclusive clustering about the gas of lower ionization potential, since the ionization potential of CO_2 exceeds that of C_2H_2; and moreover the 12 per cent. disappearance of CO_2 exceeds the prediction even had 1 CO_2 been removed for each cluster of 20 C_2H_2 molecules.

Our inability to fasten any exceptional behavior upon CO_2 and N_2 as catalysts leaves all the more puzzling their failure to auto-catalyze reactions in which they are generated, as in the decomposition of NH_3 or of CO. Both of these decompositions have just been found to be normally catalyzed by neon, so that further work will be required to explain the behavior of CO_2 and N_2, which now appears quite anomalous in these particular cases.

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[1] R. B. Moore, *J. Chem. Soc.*, Lond., 93, 2181 (1908).

[2] We are indebted to Professor K. T. Compton and Dr. Barton for some of the latest values in advance of their publication.

THE NEW STATUS OF NET ENERGY DETERMINATION

ATTENTION was called, in the April 18, 1924, number,[1] to the net energy conception of Armsby, as the

[1] Forbes, E. B., "The Net Energy Conception," SCIENCE, April 18, 1924, Vol. LIX, No. 1529, pages 350–351.

simplest and most inclusive of all general measures of nutritive value; and resolutions of the American Society of Animal Production endorsing Armsby's conception of net-energy values were quoted.

At a later date notice was given, in the June 6, 1924, number,[2] of a forthcoming paper[3] correcting all the net-energy values of feeds for cattle which had been published from this institute, on account of certain advances in understanding and method of determination; and now, as a result of further progress in our studies of the respiration calorimetry of cattle, and in view of the general interest in net energy as a scientific measure of nutritive values, we take this occasion to enumerate all the recent changes, and to indicate their effects in relation to general procedure and to accuracy in net-energy determination.

The Fasting Katabolism as a Measure of the Maintenance Requirement of Energy

It has been the practice at this institute to determine the maintenance quota of energy by Armsby's heat-increment method, that is, by comparing the heat production at two levels of feed intake, relating the difference in heat outgo to the difference in feed, and computing to the heat production on a fasting basis.

Armsby followed this procedure because he believed that it was impracticable to determine the fasting katabolism directly. We find, however, that it is entirely feasible directly to determine the fasting katabolism, and the data now at hand indicate that this value is a much higher and more nearly constant figure than is the computed fasting katabolism.

We have considered several possible explanations of this higher directly determined value, and further data may add to our understanding, but at its face value the present evidence seems clearly to indicate that the difference between the directly determined fasting katabolism and that derived by computation from super-maintenance feeding periods signifies a more efficient or economical utilization of energy below than above maintenance.

In this light, employing the directly determined fasting katabolism as the measure of the maintenance requirement of net energy, this raising of our figures for maintenance requirement has the effect to reduce the apparent difference between the heat production of fasting and at a maintenance level of feeding, while it leaves unchanged, of course, the difference between the heat production at maintenance and at full feed.

In other words, this change of standard as to maintenance requirement of energy implies decreased heat increments, and increased net-energy values, from fasting up to maintenance, the corresponding values above maintenance remaining unchanged. It seems to be necessary, therefore, to establish new net-energy values for maintenance distinct from and higher than those for growth (including fattening), and for milk production.

It is our expectation that net-energy values for maintenance and for the various types of production may be determined in each case directly, or, after direct determination for one purpose the values for the other purposes may be derived by the use of established factors.

Incidentally, this important change promises to relieve us from a situation of great embarrassment on account of the variability of repeats in the published net-energy determinations, due to the mixed effects of different proportionate amounts of sub-maintenance and supermaintenance heat increment.

Correcting the Observed Heat Production to a Standard Day, as to Standing and Lying

Since the energy cost of standing is materially higher than that of reclining, it is necessary in order to render experimental data comparable, to compute the daily heat production to a standard day, as to standing and lying; and the standard arbitrarily adopted was twelve hours' standing and twelve hours' lying.

The older method for making this correction was based on the observed differences in heat emission by radiation and conduction, while the subject was in the standing and the lying positions. These observed differences in heat emission, however, are in reality determined not only by the relative intensity of metabolism, but include also heat stored in the platform upon which the animal lies, and radiated when the animal stands. In addition, the heat absorbed by feed and water introduced, and the heat given off by the cooling of the excreta, contribute further errors; and, still further, it is entirely impracticable to separate the latent heat of water vapor emitted, on this basis. The combined effect of all these factors rendered the separation of the heat production according to intervals of standing and lying, by the earlier method, quite definitely erroneous.

We now accomplish the purpose of this separation by a much simpler and more nearly correct procedure. Thus the observed heat production is corrected to the standard day by the use of a factor representing increase in heat emission due to standing, this factor having been experimentally determined with a fasting animal. According to this new method the

[2] Forbes, E. B., "Correction of Net Energy Values," SCIENCE, June 6, 1924, Vol. LIX, No. 1536, page 511.

[3] Forbes, E. B., and Kriss, Max, "Revised Net-Energy Values of Feeding Stuffs for Cattle," *Journ. Agr. Research*, Vol. XXXI, 1925, No. 11, pages 1083 to 1099.

only data necessary to the computation of the heat production of the standard day are the total heat production, the weight of the animal, the time spent standing and the factor representing increase in heat emission due to standing.

IMPROVEMENTS OF TECHNIC

In addition to the above-mentioned considerations numerous minor improvements of technic have had the effect greatly to increase the accuracy of the experiments upon which the net-energy determinations are based, thus:

(1) The digestion period has been increased from ten to eighteen days, the calorimeter period from two to three days, and the preliminary period in the calorimeter from four and one half to thirteen and one half hours;

(2) The fact that refusal of feed can not be compensated for is now recognized, and the occasion for such corrections is avoided;

(3) The fact that the previously customary shearing of the steers at the beginning and at the end of an experimental program (to account for growth of hair) has the effect to raise the critical temperature of fasting to a point above the maximum temperature attainable with our calorimeter, as now equipped, and consequently that the animal must not be shorn, has been experimentally demonstrated;

(4) An improved insulation of the movable shield, in the calorimeter, which regulates the exposure of the cooling coils to the air of the chamber, has practically eliminated the need for changes in rate of cooling-water flow in the course of a seventy-two-hour calorimeter period, thus increasing, on several accounts, the accuracy of the heat measurement;

(5) A provision for the moistening of the air before it goes into the Bohr meters, thus preventing the lowering of the water-level in the meters, by evaporation, increases the accuracy of the air measurements;

(6) There have been improvements of carbon, nitrogen and moisture control as follows: by the use of refrigeration is prevented (a) the loss of carbon and nitrogen from the daily aliquots of feces during the course of an eighteen-day experiment; and (b) the loss of carbon from feces samples retained for the furnace determination of carbon, in the fresh material; and (c) the preservation of feeds by refrigeration makes possible the analysis of the same from the fresh condition, working to a dry instead of to an air-dry basis;

(7) The use of the new Wiley mill makes possible an improved moisture control in feeds by reason of rapid grinding, with greatly reduced attrition; and,

(8) An improved control of carbon loss from excreta during drying in an air oven, preliminary to bomb estimations of energy and carbon, has been attained by improved conditions of drying, checked by furnace determinations of carbon on the fresh materials.

In view of these very material improvements of understanding and method the idea of determining significant and useful net-energy values of feeding stuffs seems to be in a greatly improved status, and work on this project is going forward rapidly.

The writer is pleased to acknowledge that the credit for these several improvements is largely due to the cooperating members of the staff of the institute, namely, J. August Fries, W. W. Braman, D. C. Cochrane, Max Kriss, C. D. Jeffries, R. B. French, R. W. Swift and J. V. Maucher.

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SOME ECOLOGICAL RELATIONS OF THE HYPOGAEOUS FUNGI

THE following notes were made by the junior author at the suggestion of the senior author in connection with a study of the various types of peridial structure shown by California species of *Hysterangium*. The statements are largely in the form submitted by the junior author. The senior author has edited and added to these statements.

The occurrence of hypogaeous fungi of various sorts (Tuberales, Hymenogastrales, Endogonaceae, Elaphomycetaceae, etc.) in surprising number and variety in the coastal region of western North America from central California northward naturally leads to the query as to why this region is seemingly so particularly adapted to the growth of these organisms. A similar condition has been noted by Thaxter for the coast of South America along the straits of Magellan (*cf.* Thaxter, *Bot. Gaz.*, 50: 432, Dec., 1910).

At the suggestion of the senior author, his co-author made some observations tending to throw light on the climatic-edaphic-biotic complex existing in her collecting district, which is one yielding abundance of material. The following statements prepared by her refer most particularly to her experience with members of the genus *Hysterangium*:

Practically all the collecting reported on was done on King's Mountain in the Sierra Morrena section of the Santa Cruz Mountains, directly west of Redwood City, California.

The vegetation in this region is typical of a mixed forest association, being made up mostly of Redwood (*Sequoia sempervirens*), Fir (*Pseudotsuga taxifolia*), Tan Oak (*Lithocarpus densiflora*), Madrone (*Arbutus Menziesii*), Manzanita (*Arctostaphylos Manzanita*),

Live Oak (*Quercus agrifolia*) and California Lilac (*Ceanothus sorediatus*).

This region is subject to heavy, frequent coast fogs, common to most of the coast localities. It is so situated that the trees catch and hold the fog. These coast fogs usually last three days, during which time the moisture continually drips from the trees in amounts comparable to our very light spring showers. Sometimes the moisture barely penetrates the leafy surface mat, but usually a three-day fog will penetrate an inch or more into the ground.

Hysterangium is found most abundantly under tan oak or a mixed stand of tan oak and madrone. Seldom was a collection made under a pure redwood stand—the oak and madrone seem to furnish the necessary cover mat of decaying leaves. This mat seems to aid in moderating both the temperature and the moisture conditions of the soil. It should be noted here, however, that the mycelium of *Hysterangium*, no matter how near the surface of the ground it may grow, is confined to the soil and is not found penetrating the leafy cover mat. There seems, however, to be an association between the mycelium and the very fine roots found in the ground. Whether or not this may be an indication of parasitism is still an open question. Moisture and temperature conditions seem to determine largely the growth of *Hysterangium*. While I found it continuously throughout the year, the ease in locating a live patch of mycelium or one that was fruiting varied greatly.

As has been intimated, the coast fogs in that locality are frequent and continuous throughout the year. After a so-called three-day fog the mycelium was always snow-white and fruiting. It was in collections of this sort that the young stages of the sporocarp were always found. This proved to be the case, whether the collecting was done in wet April or dry September. Mature sporocarps were available at all times.

Temperature readings were made of both soil and air at the time of collecting. Collections ranged from April, 1924, to May, 1925. The following data were obtained from the readings:

Date	Time	Air	Soil
May 30	7:00 A. M.	60° F.	.....
	8:00	.....	57° F.
31	7:20	59	.....
	8:20	63	.....
	9:20	.....	52
	4:00 P. M.	70	.....
	4:30	.....	56
June 1	7:20 A. M.	69	.....
	8:30	76	.....
	10:00	.....	54
	12:30 P. M.	81	.....
	2:00	.....	57
	4:20	78	.....
July 4	4:30	49	.....
	4:00	.....	54
5	9:45 A. M.	50	.....
6	10:30	60	.....
	10:45	.....	54
Aug. 23	9:30	64	.....
	10:00	.....	56
	1:45 P. M.	.....	56
	2:30	69	.....
	4:30	74	.....
Sept. 27	10:30 A. M.	.....	56
	10:45	68	.....
Oct. 12	9:30	52	.....
	10:00	.....	50
	12:45 P. M.	58	.....
	1:05	.....	50
	1:30	58	.....
	1:50	.....	50
Nov. 30	1:30	50	.....
	2:00	.....	54
Dec. 17	10:30 A. M.	46	.....
	11:00	.....	42
Jan. 2	12:30 P. M.	52	.....
	1:00	.....	48
10	4:00	50	.....
	4:30	.....	44
31	2:00	58	.....
	2:30	.....	56
Mar. 7	4:10	.....	48
	4:30	44	.....
	4:50	.....	48
8	8:30 A. M.	42	.....
	11:20	.....	48
	11:40	46	.....

From the above table the following summary was evolved:

	Maximum			Minimum			Aver.
	Temp.	Time	Date	Temp.	Time	Date	Temp.
Air	81°F.	12.30 P.M.	6/1/24	42° F.	8.30 A.M.	3/8/25	59.2° F.
Soil	57	8.00 A.M.	5/30/24	42[1]	11.00 A.M.	12/17/24	52
				48	1.00 P.M.	1/2/25	
					4.50	3/7/25	
					11.20 A.M.	3/8/25	

[1] This low temperature was probably due to the abundance of frost which covered the ground at the time of that collection.

Moisture content of the soil was also determined for a few of the collections. This problem was more difficult to solve with accuracy than that of temperature, because soil samples were necessarily collected on Saturday or Sunday and held over until Monday for weighing. This was done, however, with a great amount of care and the samples kept in a place as free from moisture changes as possible during that time.

Moisture content varied from .0591 to .1792. The average was .10276.

This region varies greatly in the texture of its soil, which belongs to the rough, broken phase of the Altamont series and is made up mostly of loam and clay loam constituents. When there was an abundance of stages of sporocarps of *Hysterangium*, I found that the soil always felt cold and moist.

The experience of the collector, as given in the foregoing statements, seems to indicate with clearness and approximate exactitude the conditions in this one exceedingly favorable locality for hypogaei. The temperature of the soil varies around 52° F., from 42° F. as minimum, to 57° F. as maximum, but largely oscillating between 48° F. and 56° F. (8.9° C. and 13.3° C.).

The production of young sporocarps, as well as seeming invigoration of the mycelium, seem directly connected with variation in the moisture supply and to be associated particularly with fog conditions.

The edaphic conditions, while only incidentally considered, are nevertheless definite, *viz.*, coarse, well-drained soil, protected from too rapid change in temperature and moisture content by a leafy cover mat.

The biotic conditions indicate association with a broad leaf forest association, the oak component probably being particularly influential in the case of the species of *Hysterangium* studied.

While there are doubtless variations from these conditions, at least in small detail, for individual species and genera of the hypogaeous fungi of our region, yet these general conditions are probably approximate for all.

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SOCIETIES AND ACADEMIES

AMERICAN SOCIETY OF NATURALISTS

THE forty-third meeting of the American Society of Naturalists was held in New Haven, December 28 to 30, 1925. The program of the first two days was arranged in cooperation with the joint genetics sections of the Botanical Society of America and American Society of Zoologists, whose official meeting was held in Kansas City. Twenty-nine papers on genetic subjects were presented. On Wednesday the annual symposium was held, the subject being "The Structure of Protoplasm." It included the following contributions:

Some colloidal aspects of protein: PROFESSOR W. A. PATRICK, The Johns Hopkins University.

The physical state of protoplasm: PROFESSOR ROBERT CHAMBERS, Cornell University Medical College.

Newer aspects of the alveolar structure of protoplasm: PROFESSOR E. B. WILSON, Columbia University.

Some physiological aspects of the problem of protoplasmic structure: DR. W. J. V. OSTERHOUT, The Rockefeller Institute for Medical Research.

Elasticity as an indicator of protoplasmic structure: PROFESSOR WILLIAM SEIFRIZ, University of Pennsylvania.

The structure of protoplasm in Amoeba proteus: PROFESSOR S. O. MAST, The Johns Hopkins University.

The physical structure of the protoplasm of sea urchin eggs: PROFESSOR L. V. HEILBRUNN, University of Michigan.

Surface film theory of the function of mitochondria: DR. E. V. COWDRY, The Rockefeller Institute for Medical Research. (By title.)

The Naturalists' dinner was given in the Hotel Taft. After the dinner, the president, Dr. C. Hart Merriam, related many of the beliefs of California Indians about animals.

New members were elected as follows:

C. O. Appleman, University of Maryland; Ralph E. Cleland, Goucher College; F. E. Clements, Carnegie Institution of Washington; E. V. Cowdry, Rockefeller Institute; C. Stuart Gager, Brooklyn Botanic Garden; E. W. Gudger, American Museum of Natural History; Lewis V. Heilbrunn, University of Michigan; S. O. Mast, Johns Hopkins University; H. A. Pilsbry, Academy of Natural Sciences (Phila.); Inez Whipple Wilder, Smith College; A. H. Wright, Cornell University.

Professor J. Arthur Harris, University of Minnesota, was elected president for 1926; Dr. J. A. Detlefsen, editor of *Biological Abstracts*, vice-president; A. Franklin Shull, University of Michigan, secretary, and E. W. Sinnot, Connecticut Agricultural College, treasurer.

A. FRANKLIN SHULL,
Secretary

AMERICAN MATHEMATICAL SOCIETY

THE thirty-second annual meeting of the American Mathematical Society was held in New York City, from Thursday to Saturday, December 31, 1925, to January 2, 1926. The regular sessions were held at Hunter College, preceded on December 31 by the annual meeting of the board of trustees, held at the Faculty Club of Columbia University. This meeting was made especially interesting by the exhibition at Hunter College of an important part of the collec-